Carnegie Mellon
Tepper
SCHOOL OF BUSINESS

DISSERTATION

Submitted in partial fulfillment of the requirements for the degree of

DOCTOR OF PHILOSOPHY
ECONOMICS

Titled
"ESSAYS ON THE PROVISION AND FUNDING OF PUBLIC GOODS"

Presented by
Dario Cestau

Accepted by

Co-Chair: Prof. Dennis Epple 4/28/14 **Date**

Co-Chair: Prof. Richard C. Green 4/28/2014 **Date**

Approved by The Dean

Dean Robert M. Dammon 4/29/14 **Date**

Dissertation

Dario Cestau

Essays on the Provision and Funding of Public Goods.

Dissertation Abstract

Dario Cestau

Essays on the Provision and Funding of Public Goods.

The first essay studies how political parties' choice of public good provision and tax funding affect the risk of default to public debt investors. Past research has largely ignored the effects that political parties have on default risk of state governments. The objective of this paper is to address this policy question using data from Credit Default Swap contracts (CDS), and poll data from state gubernatorial elections. The findings of the paper suggest that state Republican governors have a significant positive effect on CDS spreads. On average, Republican governors reduce credit spreads by around six percent, more than half of CDS standard deviation during election race. Prospects of a Republican administration are good news for debtholders. The positive effect of Republican candidates is larger when: candidates signed the ``Taxpayer Protection Pledge'', Democrats control the state houses and for highly contested gubernatorial elections.

The second essay studies profiling and affirmative action in the access to gifted programs, a common public good provided by school districts. For decades, colleges and universities have struggled to increase participation of minority and disadvantaged students. Urban school districts confront a parallel challenge; minority and disadvantaged students are underrepresented in selective programs that use merit-based admission. We analyze optimal school district policy and develop an econometric framework providing a unified treatment of affirmative action and profiling. Implementing the model for a central-city district, we find profiling by race and income, affirmative action for low-income students, and no affirmative action with respect race. Counterfactual analysis reveals that these policies achieve 80\% of African American enrollment that would be could be attained by race-based affirmative action.

The third essay studies a new alternative mean of funding for States and local authorities called Build America Bonds (BAB). BABs were issued by municipalities for twenty months as part of the 2009 fiscal package. Unlike traditional tax-exempt municipals, BABs are taxable to the holder, but the Treasury rebates 35% of the coupon to the issuer. The stated purpose was to provide municipalities access to a more liquid market including foreign, tax-exempt, and tax-deferred investors. We find BABs do not exhibit greater liquidity than traditional municipals. BABs are more underpriced initially, particularly for interdealer trades. BABs also show a substitution from underwriter fees toward more underpricing, suggesting the underpricing is a strategic response to the tax subsidy.

Index:

Chapter 1:

Political Affiliation and Default: Are Republicans or Democrats Perceived as More Fiscally Responsible?

Political Affiliation and Default: Are Republicans or Democrats Perceived as More Fiscally Responsible?

Dario Cestau

Carnegie Mellon University

Abstract

Past research has largely ignored the effects that political parties have on default risk of state governments. The objective of this paper is to address this policy question using data from Credit Default Swap contracts (CDS), and poll data from state gubernatorial elections. The findings of the paper suggest that state Republican governors have a significant positive effect on CDS spreads. On average, Republican governors reduce credit spreads by around six percent, more than half of CDS standard deviation during election race. Prospects of a Republican administration are good news for debtholders. The positive effect of Republican candidates is larger when: candidates signed the "Taxpayer Protection Pledge", Democrats control the state houses, and for closely contested gubernatorial elections.

1 Introduction

Are Republican governors good news for holders of state debt, or is political affiliation irrelevant? This paper is an empirical study of the impact that election outcomes have on state credit spreads. I use Credit Default Swaps data (CDS)[1] to obtain a proxy of states' risk of default, and collect polls data from 136 pollsters to measure the likelihood of gubernatorial election outcomes. I measure political parties' effects by considering the response of CDS spreads to movements in poll predictions during a gubernatorial election race. If an increase in the probability of victory translates into a decrease in credit spreads, the political party is good news for debtholders. I find that, at the state level, an increased likelihood of a Republican victory is associated with a decrease in credit spreads. On average, Republican governors reduce credit spreads by around six percent, which is equivalent to more than half of the average CDS standard deviation during gubernatorial election races.

A placebo regression test shows that the analysis yields consistent estimates of political party effects. The results in this paper are also robust to alternative dependent and independent variables, and to an event case study. I also present evidence that the effect gets stronger for highly contested gubernatorial elections. Additionally, I find that debtholders do not have a preference for divided government, however we observe a higher reduction in credit spreads when Democrats control the state legislative branch. This result is consistent with the veto power that all state governors share in the sample. An ability to veto becomes relevant when the opposing party has majority control of state houses. The positive effect of Republican candidates is even stronger when candidates signed the "Taxpayer Protection Pledge", that is a pledge to oppose increases in marginal income tax rates for individuals and businesses.

[1]A CDS is a financial contract that works like an insurance contract. In case of default by the State authority the protection seller buys the defaulted bonds at par value from the protection buyer. The protection buyer pays a quarterly fee known as CDS spread for this insurance.

A long list of researchers have studied the effect of political parties on public spending and tax levels, but not on credit risk. On the spending side, Rogers and Rogers (2000) and Besley and Case (2003) find that a higher fraction of Democrats in the state House is associated with larger expenditures. Alt and Lowry (1994) asserts that Democratic state governments are inclined to produce higher public spending as a share of per capita income. According to Reed (2006) Democratic control leads to a government size increase of the order of 3-5% compared to Republican control. However, previous research also finds effects that are more mixed. Garand (1988) find inconclusive evidence that party control affects public spending. Besley and Case (1995b) find that Democratic governors increase public spending in the last term. Gilligan and Matsusaka (1995) find small party effects on spending at the state and local levels. On the tax side Besley and Case (1995a and 1995b) find that Democrats facing term limits increase taxes, but there are no differences between political parties otherwise. Knight (2000) shows that taxes drop with Republican control of both states Houses, while the opposite is true for Democratic control. Caplan (2001) finds that higher Democratic representation in either state legislative Houses is associated with tax increases. Alt and Lowry (1994) also contends that Democrats increase state taxes as a share of per capita income.

To the best of my knowledge, no study has attempted to answer which political party increases governments' ability to repay debt. This paper contributes to the literature by providing an empirical answer to this question. Economic shocks simultaneously affect election outcomes and fiscal outcomes. The literature in political economy often employs regression discontinuity analysis to obtain unbiased estimates[2]. The contribution of this paper includes an alternative to this method. It obtains unbiased estimates by using high frequency financial data and a common assumption in the literature: high frequency changes in poll intentions are exogenous

[2]Recent literature such as Lee (2008), Fredriksson, Wang and Warren (2009) and Ferreira and Gyourko (2009) has dealt with it by using regression discontinuity design analysis. Lee (2007) establishes weak conditions to justify a sharp regression discontinuity design by arguing that close decision elections have enough random variation in their outcomes.

to high frequency economic variables. An additional contribution to the literature is to include an original adaptation of placebo regression analysis to panel regression analysis. This work also makes use of a novel database for state CDS and it constructs a unique database of poll predictions for gubernatorial elections.

The results in this paper are important for finance both in demonstrating that political affiliation matters in markets for government debt and in quantifying the magnitude of the effects. They are also important to the quest in political economy to understand the effects of political parties on economic outcomes. This work is valuable from an interdisciplinary perspective in by bringing knowledge of financial markets and financial instruments to the study of issues of importance in economics and other social sciences.

Although prospects of a republican administration decreases credit risk it does not necessarily imply that republican governors are more successful in balancing their tax and spending policies. Several explanations may account for a credit risk drop. Republican governors might be debtholder friendlier and exert higher efforts to repay debt. They might execute more effective policies to increase state output and consequently tax revenues. They might favor fiscal austerity, or a combination of the three factors above. While I cannot resolve the precise mechanisms through which Republican governors decrease credit risk, I do make an attempt to provide insight on the subject by constructing a profile that characterizes republican candidates:

Every Republican candidate campaign in my sample was characterized by promises to not increase taxes. Half of them even signed the "Taxpayer Protection Pledge". Coincidentally or not, some candidates repeated the phrase once spoken by George Bush senior: "Read my lips: no new taxes". In every case, a promise to not increase taxes came matched with a promise to reduce government size. Both policies are a sign of fiscal austerity. Democratic candidates did not make opposite promises, but they did not commit to reduce taxes or spending. They centered their campaign promises someplace else. By focusing on those election races where a candidate signed the pledge, the drop in credit spreads is even larger. This might suggest that fiscal austerity is the cause driving debtholders high hopes.[3]

The remainder of the paper proceeds as follows. Section 2 briefly describes state default risk. Section 3 introduces the empirical strategy employed to estimate political party effects. Section 4 offers a detailed description of the data. Section 5 describes the empirical findings. Section 6 presents several robustness checks. Section 7 tests some common conjectures. Section 8 concludes.

2 State Default Risk and CDS spreads

2.1 Default Risk

Several states have defaulted in U.S. history. Ang and Longstaff (2013) presents a detailed summary of state defaults. Nine states and one territory defaulted during the first half of the 19th century, and ten states in the second half of the 19th century. A single default took place in the 20th century. Although no defaults have taken place since Arkansas during the Great Depression, some states have been close to default.

Before the financial crisis, most municipal debt was issued with insurance and thus state default risk was less relevant. The financial crisis was a turning point in the market. As a consequence of a falling fraction of insured municipal debt and worsening fiscal conditions, state default risk started to be a tangible risk for investors

[3] The greater ability to repay debt does not seem be a consequence of a faster economic growth. There is literature like Santa-Clara and Valkanov (2003), Knight (2006) and Alesina and Sachs (1986), that finds that stock market returns and output are positively correlated with Democratic governments

and CDS contracts began to trade. In the subsequent four years the market for CDS expanded to include more sovereign state debt. State debt was not riskless and credit risk premiums paid by investors were not negligible. Between 2008 and 2012, average CDS spreads were three times larger for states than for the federal government.

2.2 CDS spreads vs. Bond Yields

It was with the advent of CDS contracts that good state credit risk data became available. Credit risk is priced into bond yields and CDS spreads. Duffie (1999) shows that under certain conditions, credit spreads are equal to CDS spreads. Thus, both types of securities can be used to obtain a measure of default risk. Nonetheless CDS spreads present two key advantage over bond yields.

First, there is generally no bond yield data available during a gubernatorial race. New issues present a low number of trades that are mostly concentrated over the first trading days, with very little trading activity afterwards.[4] As a consequence, there is no data available in the absence of a new issue during an election race. On the other side, CDS contracts present longer data series that include periods of gubernatorial races. Second, CDS spreads provide a more direct measure of credit risk than yield spreads. A bond yield reflects the credit risk of a single bond, while CDS contracts reflect the credit risk of the pool of bonds issued by a state authority. Any default on any bond in this pool of bonds triggers CDS contract payments.

2.3 CDS Liquidity

Although CDS contracts are more liquid than state bonds, liquidity remains a concern. Low liquidity downwardly biases measures of political party effects on credit

[4] Low trade frequency for state bonds has been documented by Green, Hollifield and Schurhoff (2007a and 2007b) and Cestau, Green and Schurhoff (2013)

risk since it is less likely that CDS spreads respond to movements in poll predictions.

There is no volume data since it is an over-the-counter market. Nevertheless we observe daily price movements. Besides frequent price movements, liquidity measured as the mean bid-offer spread, is comparable between states and countries, with the former being just 20% larger.[5]

3 Empirical Strategy

I consider every state gubernatorial election that took place between 2008 and 2012 for which CDS and poll data are available, and every state with CDS data but with no poll data during this period. State CDS data is not available before 2008. Between 2008 and 2012, gubernatorial elections were carried out in 2008, 2009, 2010 and 2012. I call them election years. I use panel regression analysis with fixed effects by election year and heteroscedastic errors clustered by election year. The frequency or unit of time in the panel model is a week. My dependent variable is the five-year state CDS spread. The independent variable is the Republican share of the total predicted votes for Republicans and Democrats at gubernatorial election polls[6]. I add a set of domestic and international economic variables to capture CDS spread movements caused by economic outcomes. I add time fixed effects to capture CDS movements caused by variables with a frequency of less than a week.

I take first difference to both sides of the equation to avoid cointegration, as I do not reject a test for unit root for the dependent and independent variables. My model explains weekly differences instead of levels. At any time in any election year, states face different probabilities of a republican administration (call it "treatment"),

[5] 15% state CDS and 12.3% country CDS.

[6] If a pollster predicts: 38% Republicans - 57% Democrats then the Republican Share= $38\%/(38\% + 57\%) = 38\%/(95\%) = 0.40$

or no probability at all if there is not an election race going on (I assume a constant probability). Thus, some researchers calls it a difference in difference in difference estimator, others just calls it a difference in difference.

The panel regression model is specified as follows,

$$\Delta S_{s,t} \;=\; \alpha_s \;+\; \beta_0 \Delta Sh(Republican)_{s,t} \;+\; \Delta X_t^{'}\theta_t \;+\; \gamma_t \;+\; \epsilon_{s,t} \qquad (1)$$

where s indices states and t denotes time. The dependent variable $\Delta S_{s,t}$ denotes weekly changes in five years CDS spreads measured in basis points. The independent variable $\Delta Sh(Republican)_{s,t}$ denotes the weekly change in poll predictions. X_t is a vector of high frequency control variables, α_s are election year fixed effects, γ_t time fixed effects and $\epsilon_{s,t}$ is a white error term. β_0 and θ are parameters to be estimated, where β is our parameter of interest. The parameter β is common for every state since I am interested in the average effect of the Republican Party in credit spreads.

To increase the accuracy of my estimates I control for domestic and international state of the economy. I include the controls used in Ang and Longstaff (2013), along with others. I use 4 groups of variables: economic performance variables, company CDS indices, sovereign CDS indices, and sovereign CDS contracts. The first group contains the S&P500, DAX index and the VIX index. The second group includes the following indices: North America Investment grade CDSs index (CDX.NA.IG), Europe Benchmark CDSs index (iTraxx Europe), Asia excluding Japan Investment Grade CDSs index (iTraxx Asia) and Japan CDSs index (iTraxx Japan). The third one is a one-element group consisting of the Emerging Market CDSs index (CDX.EM). The fourth group is formed by G-8 sovereign CDSs plus CDSs of five other major economies: Brazil, China, Indonesia, Mexico and Russia. Canada is excluded due to unavailability of CDS data.

3.1 Consistent Estimates

The vast majority of voters do not systematically use high frequency variables to weekly update their preference for candidates. This is why poll predictions are often used as instrumental variables in research that studies political candidate effects on firm returns.[7] Most variables that affect both CDS spreads and political preferences come in the form of low frequency data, such as state annual reports, state total debt, state debt to total property value, public employment rate, unemployment rate, per capita income, debt as a percentage of state revenues, and many others. Their effects on CDS spreads are captured by time fixed effects from a difference in difference in difference model. Thus, they do not bias the results.

Lets assume there exists a high frequency variable used by investors to price weekly CDS spreads and also used by voters to update their preferences every week. There are two possible scenarios: that it is independent between states or that it is correlated between states. In the latter case we can assume it is the same variable for every state.

In the first case it creates a bias on individual candidate effects, however it will average out as they are independent events[8]. Therefore, one obtains unbiased average political party effects by using panel analysis. In the second case it will not average out and it is necessary to test for spurious correlation. To check whether there is true causation I perform a placebo panel regression test, which I later explain in detail.

I argue that reverse causation is unlikely. It seems implausible that voters form their weekly preference over candidates after observing the weekly change in CDS spreads. The vast majority of voters are not sophisticated enough to know about

[7]A sample of papers that uses polls as instrumental variables are Herron et al. (1999), Knight (2006), Snowberg, Wolfers and Zitzewitz (2006) and Mattozzi (2010)

[8]By the law of large numbers

CDS spreads, and if they are, this data is not readily available to them in real time.

4 Data

4.1 CDS Contracts.

CDS securities may be issued in multiple currencies. For some countries, the notional amount in one currency may be as important as in another currency. The general case however, is that one currency dominates over the others. For example, most U.S. CDS contracts are denominated in Euros, but they are found in other currencies as well. Although the choice of currency should not be important (since credit spreads have no denomination), it is important to use the currency with most liquidity.

CDS contracts use four credit event definitions: "CR"[9],"MR"[10], "MM"[11] and "XR"[12]. The standard definition changes from region to region, type of reference entity, and type of underlying asset. For example, most but not all California CDS contracts use a "CR" definition, while most investment grade European indices use a "MM" one. It is important to choose the credit event definition that is the most liquid for for every region and type of contract.

I obtained CDS data from 3 sources: Bloomberg, Datastream and CMA datavision. I use five-year maturity contracts that are known to be the most liquid. I relied on the following criteria to select the currency and credit event for each variable: longest time series in the three sources, highest variability and fewer differences be-

[9] "CR" - Complete Restructuring: Any restructuring event qualifies as a credit event and any bond with up to 30 years maturity is deliverable

[10] "MR" - Modified Restructuring: Restructuring agreements count as a credit event, but the deliverable obligation against the contract has to be limited to those instruments with a maturity of 30 months or less after the termination date of the CDS contract or the reference obligation that is restructured (regardless of maturity)

[11] "MM" - "Modified-Modified" restructuring: Like "MR" but with a maturity limit of 60 months for restructured obligations and 30 months for all other obligations.

[12] "XR" - No Restructuring: All restructuring events are excluded as trigger events.

tween the 3 sources. The reason behind high variability is that once a CDS or CDS index is obsolete, trade frequency decreases and quotes tend to remain unchanged for longer periods. The reason I use time series that are similar between sources is obvious; it reduces the likelihood of typographical errors. The following table summarizes the currency denomination and credit event definition for each variable.

Table 1: Variable choice

	Currency	Credit Event
State CDS	Dollar	CR
U.S. CDS	Euro	CR
Countries CDS	Dollar	CR
CDX index E.M.	Dollar	CR
CDX index I.G.	Dollar	XR
Itraxx Europe	Euros	MM
Itraxx Asia	Dollar	MM
Itraxx Japan	Yen	CR

Five-year midmarket quotes were obtained from Bloomberg since it has the longest time series and has reliable daily data under the criteria described above. I also collected ten-year CDS spreads data from CMA datavision. Datastream data was mostly incomplete. It was useful to verify the accuracy of the other two sources. Data for VIX, S&P 500, and DAX indices were obtained from Datastream. I use end of week values, and take Friday-to-Friday differences for every CDS and market variable.

4.2 Poll data

I collected polls data on gubernatorial races for each state for which I have CDS prices. As mentioned before, CDS is a new kind of financial instrument and CDS data spans from 2008 to 2012. During this period every state had at least one gubernatorial election, most of them in 2010. There is not CDS data for every state nor there is poll data for every election during this time frame. States with both types of data are: New Jersey in 2009; California, Connecticut, Florida, Illinois, Maryland, Massachusetts, Michigan, Nevada, New York, Ohio, Pennsylvania, Texas and Wisconsin in 2010; and North Carolina, Washington and Wisconsin in 2012.

There are more than 130 pollsters for states altogether but not a complete database of gubernatorial election polls. I constructed a unique database using four different sources: FiveThirtyEight (NYtimes), Real Clear Politics, Ballotpedia and Wikipedia. From each source I collected the following data: pollster name, dates conducted, sample size and voters intentions. Even when the four sources have data for the same pollster, it is often recorded by different names in each source, and sometimes within a same source. During California gubernatorial election 2010, the University of Southern California conducted several polls. The same pollster was recorded by three different names: USC, USC/LA times and LA times. This is not a unique case. When data for the California election was brought together from the four sources, it totalled 54 different pollsters, whereas the true number of pollsters was 15. It is imperative to correct all pollster names to avoid duplicating a poll. After editing all pollster names for every state I contrasted every source to amend errors in date, sample size and vote intentions.

There is no data of when a poll prediction was publicly available. I make the simplifying assumption that poll predictions are released the day after the poll is conducted. I use the average Republican share from every poll prediction released between Saturday and Friday. I take weekly averages to avoid having spikes caused by unusual poll reports. Otherwise it would bias my estimates. I equally weight every poll prediction for the following reason: While it is true that some pollsters are more reliable than others there is no unequivocal and transparent ranking of every pollster in my sample. The reliability of a pollster does not depend on the sample size either, but on a correct statistical selection of its sample. Different sample sizes modify the margin of error without creating a bias. I do not weight by sample size because it is not unusual to find a smaller sample size poll to be more reliable than a larger one.

5 Empirical Results

This section analyzes the effect of political affiliation on state default risk. I consider every state gubernatorial election that took place between 2008 and 2012. More than half of the states do not have CDS trades for this time frame. Every state had at least one gubernatorial election during this period, most of them in November 2010, however poll data is not available for every state. I drop from the sample election races with three party contests with the exception of New Jersey 2009 election that had a weak third party for the most part. After dropping every election contest without polls data, with a three party contest, and every state without CDS data, my final sample consists of 20 states and 17 gubernatorial elections. I restrict my data to six months prior to Election Day because there is limited polls data before that date and there is too much uncertainty about primaries outcomes.

Table 2 provides summary statistics of state CDS spreads during the six months prior to Election Day. Average values are not negligible. They range between 62 and 282 basis points. That is between two to seven times the average spread for the federal government. The maximum and minimum gap averages 71 basis points and the median state standard deviation is thirteen points. All states experienced considerable variability during the period of analysis.

Table 3 shows summary statistics for the time series of Republican vote shares during the six months prior to Election Day. Mean Republican shares pivot around 50%, the cutoff point that determines the election winner. This indicates there was not a pronounced preference for Republican or Democratic candidates during the period of analysis. The difference between the maximum Republican vote share and the minimum Republican vote share for any election race is around 9%. This is a

Table 2: Summary Statistics for State CDS (in basis points)

	Mean	S.D.	Min.	Max.
N. Jersey 09'	176	52	75	235
California 10'	282	23	243	346
Connect 10'	114	13	90	141
Florida 10'	138	23	96	187
Illinois 10'	281	38	199	360
Maryland 10'	62	9	44	81
Massachu 10'	126	13	96	153
Michigan 10'	230	23	186	283
Nevada 10'	193	13	164	218
New York	222	26	185	284
Ohio 10'	129	11	106	153
Pennsylvania	126	7	119	147
Texas 10'	80	11	58	102
Wiscon 10'	118	11	93	146
N. Carolina 12'	79	10	50	95
Washing 12'	80	11	64	101
Wiscon 12'	101	14	83	126
Median	126	13	96	153

significant change when we consider that increasing the vote share by ten percent points, for example from 45% to 55%, translates into going from losing the election with almost certainty to winning the election with almost certainty. Thus, it is not unusual for election contests to have swings on the foreseeable winner. The median standard deviation is close to 2.5%, which again indicates considerable variability in poll predictions.

Table 4 shows the average effect that the Republican Party has on credit spreads. It estimates three different specifications: one without control variables, one with only North America control variables, and one with all of the control variables described in section three. Every specification includes election year fixed effects, time fixed effects and Newey-West standard errors clustered by election year. I divide the sample into two groups: a first group that contains every election contest and a second group that includes only gubernatorial elections in 2010. Each specification is estimated for both samples.

Table 3: Summary Statistics for Republican vote shares

	Mean (%)	S.D. (%)	Max-Min (%)
N. Jersey 09'	54	2.4	8
California 10'	49	2.7	8
Connect 10'	46	4.6	16
Florida 10'	50	2.7	11
Illinois 10'	55	2.0	9
Maryland 10'	47	2.5	8
Massachu 10'	46	2.6	9
Michigan 10'	61	2.6	10
Nevada 10'	59	2.2	8
New York	33	5.6	17
Ohio 10'	52	2.8	10
Pennsylvania	57	2.4	10
Texas 10'	54	1.9	7
Wiscon 10'	54	2.1	10
N. Carolina 12'	56	2.0	8
Washing 12'	49	2.1	6
Wiscon 12'	52	2.0	6
Median	52	2.4	9

Every coefficient for the Republican share is negative and statistically significant at 99% confidence. Magnitudes do not increase or decrease as I introduce more control variables. The 2010 sub-sample coefficients show similar magnitudes and same sign. These results are strong evidence that on average, the Republican Party is good news for debtholders at the state level.

Table 4 results are read in the following way: an increase in the Republican share of voters from 0% to 100% translates into an average reduction in CDS spreads of 51[13] basis point (42% decrease). But we never observe such poll movements from week to week. A more pragmatic way to interpret these results is to think of the effect of a republican victory. I transform vote shares into probabilities of winning an election on the day the poll was conducted[14].

[13] An average of the 6 estimates in Table 4

[14] They are not the actual probabilities of a republican victory at Election Day

Table 4: 5y CDS Week Last Vs. Polls Week Avg

	All	2010	All	2010	All	2010
Republican Share	-50.88***	-55.90***	-49.30***	-51.13***	-53.38***	-43.64***
	(13.07)	(13.25)	(13.51)	(11.80)	(14.73)	(10.47)
Controls:						
U.S. Economy			Yes	Yes	Yes	Yes
U.S. CDS			Yes	Yes	Yes	Yes
CDX indices			Yes	Yes	Yes	Yes
ITRX indices					Yes	Yes
OECD CDS					Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	924	203	924	203	924	203
R-sq	0.16	0.27	0.33	0.40	0.39	0.46

Robust standard errors between parentheses
* p<0.10, ** p<0.05, *** p<0.01

Table 5 shows the average effect that a Republican victory has on credit spreads when shares are expressed as probabilities. It replicates columns three and five of table 4 using a transformed independent variable. Republican candidates decrease credit risk and estimates are statistically significant at 99% confidence. A Republican candidate victory reduces CDS spreads in 7.4[15] basis point, a 6% reduction. This is an economically meaningful reduction.

Table 6 shows the savings that governments would incur if they had to rollover all of their general and revenue bonds under a Republican administration. On average, a Republican administration would save 93 million dollars every five years from compensating investors for holding state risky debt[16].

The estimates in Table 4 are conservative and in fact represent a lower bound of

[15] An average of Table 5 estimates

[16] To calculate savings I multiply total bonded debt at the time of the election (as reported by states' annual comprehensive reports) by the CDS spreads decrease found in Table 5. CDS spreads give a direct measure for risk compensation for the duration of the CDS contract

Table 5: Results' Interpretation

	Win Probability	
Republican Share	-6.78***	-8.03***
	(1.61)	(1.25)
Controls:		
U.S. Economy	Yes	Yes
U.S. CDS	Yes	Yes
CDX indices	Yes	Yes
ITRX indices		Yes
OECD CDS		Yes
State Fixed Effects	Yes	Yes
Month Fixed Effects	Yes	Yes
N	924	924
R-sq	0.33	0.39 1

Robust standard errors between parentheses
* p<0.10, ** p<0.05, *** p<0.01

Table 6: 5 years Savings

	Savings ($)
California	412,360,328
New York	204,689,010
New Jersey	136,661,848
Texas	132,153,333
Florida	110,127,160
Illinois	106,440,333
Massachusetts	83,149,379
Washington	68,225,968
Connecticut	67,249,526
Ohio	61,598,174
Wisconsin	44,283,862
Pennsylvania	38,871,785
North Carolina	30,826,286
Maryland	30,242,879
Michigan	26,172,973
NEvada	14,201,157
Average	93,265,966

party effects on credit risk. First, the effect may be decreasing as we move away from 50% share, but I am equally weighting every gubernatorial election. The response of CDS spreads to changes in poll predictions may be smaller in some elections where party differences are so big that investors do not perceive a change in predicted vote shares as a source of meaningful changes in the expected victory. Table 5 solves part of the problem, but it assumes that the election take place the same day a poll is conducted, which is not the case. Later in the paper I drop those "uncontested" elections and it significantly increases the measured effect. Second, the change in investors' expectations about the election winner becomes more responsive to poll predictions as we move towards the Election Day, where poll accuracy improves. However I equally weight poll changes over time, which overestimates the change of an expected winner and underestimates the effect on CDS spreads.

6 Robustness Checks

6.1 Placebo Panel Regression

A placebo panel regression analysis shows that poll movements are causing changes in CDS spreads and that it is not a spurious correlation.

When it comes to linear regressions, a placebo regression analysis means to estimate a linear model using an independent variable that behaves similar to the true independent variable, but with no theoretical effect. An example would be to study if CDS spreads in Wisconsin are responding to poll results in Michigan, with Michigan being the state with a governor's race while Wisconsin is not having one. I adapt this concept to panel regression analysis. I take every election in 2010 and I randomly interchange poll election data between states and estimate the model. The intuition behind this approach is as follows:

Assume there are true and positive political effects ($\beta_0 > 0$) and no missing variables in the right hand side of the equation (1). When polls go up, CDS go up. When polls go down, CDS go down too. At any moment polls are going up and down for different states, as illustrated by equation (2). An estimation of equation (1) results in $\hat{\beta} > 0$, which is correct since $\beta_0 > 0$.

$$\begin{aligned} \Delta C_{NJ} \uparrow &= \ldots \beta_0 * \Delta Sh(Republican)_{NJ} \uparrow + \ldots \\ \Delta C_{FL} \downarrow &= \ldots \beta_0 * \Delta Sh(Republican)_{FL} \downarrow + \ldots \\ \vdots &= \ldots \qquad \vdots \qquad + \ldots \end{aligned} \tag{2}$$

Interchange poll data across states as illustrated by equation (3). Notice I call β_0 just β in equation (3). Under the assumption that cross state effects are negligible then $\beta = 0$. An estimation of equation (3) results in $\hat{\beta} = 0$, which is correct since $\beta = 0$.

$$\begin{aligned}
\Delta C_{NJ} \uparrow &= \; ... \; \beta * \Delta Sh(Republican)_{FL} \downarrow \; + ... \\
\Delta C_{FL} \downarrow &= \; ... \; \beta * \Delta Sh(Republican)_{NJ} \uparrow \; + ... \\
\vdots \quad &= \; ... \qquad \vdots \qquad + ...
\end{aligned} \tag{3}$$

Now assume there is a variable "M" that is positively correlated with polls and CDS spreads. If this variable goes up, polls go up and CDS spreads go up too. It is the same for every state. Polls and CDS spreads will move in the same direction across states, as illustrated by equation (4):

$$\begin{aligned}
\Delta C_{NJ} \uparrow &= \; ... \; \beta * \Delta Sh(Republican)_{NJ} \uparrow \; + \Delta M \uparrow \; + ... \\
\Delta C_{FL} \uparrow &= \; ... \; \beta * \Delta Sh(Republican)_{FL} \uparrow \; + \Delta M \uparrow \; + ...
\end{aligned} \tag{4}$$

If this variable is not included in the right hand side of equation (1) and we interchange poll data, it is still true that a poll increase is matched with a CDS spread increase for every state. This is because polls and CDS spread are positively correlated to the same missing variable for every state[17]. An estimation of equation (4) results in $\hat{\beta} > 0$, which is a biased estimate of $\beta = 0$ and it is caused by a missing variable.

$$\begin{aligned}
\Delta C_{NJ} \uparrow &= \; ... \; \beta * \Delta Sh(Republican)_{FL} \uparrow \; + ... \\
\Delta C_{FL} \uparrow &= \; ... \; \beta * \Delta Sh(Republican)_{NJ} \uparrow \; + ...
\end{aligned} \tag{5}$$

[17] if the correlation with this missing variable is independent across states the bias will average out

After interchanging poll data across states we get $\beta = 0$ with no missing variables (even when $\beta_0 > 0$) and $\beta > 0$ with a missing variable. Poll data cannot be assigned to the originator state, e.g. I cannot assign Ohio poll data to Ohio. Otherwise, if there are political party effects ($\beta_0 > 0$), we get $\beta \neq 0$ under the null hypothesis of no missing variables. When no poll data is matched with the originator state we have that $\beta = 0$ under the null, independent of the existence or not of political party effects on CDS spreads[18]. With no missing variables, theoretical rejection rates of the null $\beta = 0$ are: 10% using a 90% confidence interval, 5% using a 95% confidence interval and a 1% using a 99% confidence interval.

I generate fifty derangements[19] and estimate three specifications for each of the fifty derangements: a first one without control variables, a second one with only North America control variables and a third one with all control variables described in section two. Table 7 compares the rejection rates for 50 derangements to theoretical rates. Rejection rates in column three are almost equal to theoretical ones. This indicates that Table 4 estimates are not the result of missing explanatory variables.

Table 7: Placebo Panel Analysis

Null Hypothesis	Significance Level	Theoretical Rejection Rates [Confidence Interval]	Simluation Rejection Rates
	99%	1% [+/-1.6]	1%
$\beta = 0$	95%	5% [+/-3.5]	5%
	90%	10% [+/-4.8]	12%
Total trials: 150			

[18]Shown by the example of equation (3)

[19]I randomly generate 50 permutations of polls data using Matlab 2013a default seed

6.2 Alternative Independent Variables

Table 8 presents four robustness checks using two alternatives of the baseline model. The first two columns replicate the baseline model of Table 4 using ten-year CDS spreads. The last two columns show event-study estimates. In this set up, a conventional event-study is not feasible since there is too much clustering of events in 2010. It is implemented by including a dummy variable with value one if a Republican candidate wins the election and -1 if a Democratic candidate wins the election. This dummy variable is the explanatory variable in columns three and four. I estimate two specifications for each alternative : a first one where with only North America control variables and a second one with the whole set of control variables.

The coefficients in the first two columns of Table 8 show similar political party effects to table 4. Coefficients are statistically significant at 99%. The last columns show the response of CDS spreads to election outcomes, ignoring information prior to Election Day. Results from the event-study also suggest that Republican governors are good news for debtholders. Significance decreases to 90%, which is expected given the information loss. At Election Day some election outcomes are foreseeable and are incorporated into CDS spreads well in advance.

Table 8: Robustness Checks

	10 Years CDS		Event Study	
Republican Share	-46.86***	-51.11***	-3.11*	-3.04*
	(10.01)	(12.67)	(1.77)	(1.64)
Controls:				
U.S. Economy	Yes	Yes	Yes	Yes
U.S. CDS	Yes	Yes	Yes	Yes
CDX indices	Yes	Yes	Yes	Yes
ITRX indices		Yes		Yes
OECD CDS		Yes		Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes
N	925	925	3356	3339
R-sq	0.35	0.41	0.26	0.31

Robust standard errors between parentheses
* p<0.10, ** p<0.05, *** p<0.01

7 Other Conjetures

7.1 Effect by level of contested elections

This section studies the conjecture that political party effects are larger for highly contested elections.

The upper panel of Table 9 shows descriptive statistics of transformed Republican shares into probabilities of victory. I use polls' sample size and a standard method to obtain a probability measure in a two-party election contest. Not every election contest has an uncertain outcome. For some gubernatorial elections the final outcome can be accurately predicted several weeks prior to Election Day. Columns one, two and three show the mean, maximum and minimum probability of a Republican victory. The first five contests in the list show almost no movement in the probability. The election outcomes are quite foreseeable in those cases. The remaining twelve contests in the list show moderate to high outcome uncertainty. The lower panel of Table 9 replicates the first column of Table 4 using a sample of "uncertain" elections. In this case the average effect of the Republican Party almost doubles the effect found previously.

Table 9: Individual State Analysis

Republican Win Probability by State				
State	Mean	Min	Max	
Michigan	1.00	1.0	1.0	
North Carolina	0.97	0.7	1.0	
Nevada	1.00	1.0	1.0	
New York	0.00	0.0	0.0	
Pennsylvania	0.98	0.9	1.0	
New Jersey	0.85	0.3	1.0	✓
California	0.32	0.0	1.0	✓
Connecticut	0.21	0.0	1.0	✓
Florida	0.50	0.0	0.9	✓
Illinois	0.80	0.0	1.0	✓
Maryland	0.19	0.0	0.8	✓
Massachusetts	0.09	0.0	0.5	✓
Ohio	0.56	0.0	1.0	✓
Texas	0.93	0.5	1.0	✓
Wisconsin 10'	0.88	0.4	1.0	✓
Washington	0.59	0.1	1.0	✓
Wisconsin 12'	0.76	0.2	1.0	✓
Panel Analysis - Checked States				
Republican Share	-82.08***	R^2	=	0.04
	(28.61)	N	=	866

I divide my sample into two subsamples. The first subsample contains the eight election contests with average Republican shares closest to 50%. The second subsample contains the eight election contests with average Republican shares most distant to 50%. If the conjecture is correct, political parties' effect should be larger for the first group. The first two columns of Table 10 show the average effect for each group. The last two columns use a dummy variable for the second group to determine whether the difference between groups is statistically significant. The first row of Table 10 shows political effects that are 40% to 80% larger to those found in Table 4. The second row shows lower effects for the least contested election. Although positive, the difference between groups is roughly significant at 90%. Overall, Table 10 shows some evidence that the effect is stronger for more contested elections. This means that table 4 estimates are a lower bound of political party effects[20].

Table 10: Effect by level of contested elections

	Levels			Difference	
Republican Share	-70.26**	-90.26***	Republican Share	-73.49**	-90.36***
(top 8 states)	(31.32)	(26.16)		(31.20)	(25.11)
Republican Share	-35.99**	-37.13*	Dummy bottom	37.14	50.88*
(bottom 8 states)	(16.05)	(18.90)	8 states	(34.24)	(30.06)
Controls:					
U.S. Economy	Yes	Yes		Yes	Yes
U.S. CDS	Yes	Yes		Yes	Yes
CDX indices	Yes	Yes		Yes	Yes
ITRX indices		Yes			Yes
OECD CDS		Yes			Yes
State Fixed Effects	Yes	Yes		Yes	Yes
Month Fixed Effects	Yes	Yes		Yes	Yes
N	821/834	821/834		902	902
R-sq	0.32/ 0.34	0.39/0.41		0.33	0.39

7.2 Divided Government

This section borrows from the literature on divided government and studies the conjecture that debt holders prefer divided governments.

There is no data that provides forecasts on which political party will win control of the state houses. Nevertheless, I observe which political party controls the state houses ex post. I make the simplifying assumption that market participants

[20] explained in section 5

can perfectly forecast who will win control of both houses close to Election Day. I divide my sample into two groups. The first group contains nine states where the Democratic Party controls both houses after election. The second group contains the eight states with the Republican Party controlling both houses after election. Table 11 summarizes which party had control over state houses in each state. New York was the only state with divided control of the state legislature. Nonetheless Democrats gained control one year after the election.

Table 11: State Houses

	Majority Party
N. Jersey 09'	Democratic
California 10'	Democratic
Connect 10'	Democratic
Illinois 10'	Democratic
Maryland 10'	Democratic
Massachu 10'	Democratic
Nevada 10'	Democratic
Washing 12'	Democratic
New York	Democratic
Florida 10'	Republican
Michigan 10'	Republican
Ohio 10'	Republican
Pennsylvania	Republican
Texas 10'	Republican
Wiscon 10'	Republican
N. Carolina 12'	Republican
Wiscon 12'	Republican

The first two columns of Table 12 show the average effect for each group. The last two columns use a dummy variable for the second group to study whether the difference between groups is statistically significant. If the conjecture is correct, the coefficients should be negative for the first row and positive for the first two columns of the second row. Table 12 shows there is not a preference for divided government since average effects are not positive for the second group. Nevertheless it shows a larger and statistically significant preference for republican governors when Democrats control both state houses. It remains to be answered whether they like the combination

"Republican Governor/Democratic legislature" or they have a strong adverse preference for Democratic legislatures[21].

Table 12: Effect by divided government

	Levels			Difference	
Republican Share (Democratic Houses)	-68.65*** (18.38)	-65.50*** (18.73)	Republican Share	-69.18*** (17.45)	-66.89*** (17.75)
Republican Share (Republican Houses)	-14.83 (19.50)	-30.60 (31.12)	Dummy Republican Houses	54.84** (25.37)	37.29 (33.68)
Controls:					
U.S. Economy	Yes	Yes		Yes	Yes
U.S. CDS	Yes	Yes		Yes	Yes
CDX indices	Yes	Yes		Yes	Yes
ITRX indices		Yes			Yes
OECD CDS		Yes			Yes
State Fixed Effects	Yes	Yes		Yes	Yes
Month Fixed Effects	Yes	Yes		Yes	Yes
N	849/828	849/828		924	924
R-sq	0.33/0.33	0.41/0.39		0.33	0.39

7.3 Candidates Profile

This section makes an attempt to determine what makes a Republican candidate attractive to bondholders. It does not seem to be a socio-demographic characteristic. Republican and Democratic candidates are around the same age, have high income and are highly educated. The main and consistent differences between them are their campaign promises about tax and spending policies. I found that every Republican candidate in my sample made promises to cut spending and not to raise taxes. Unfortunately there are no ratings for gubernatorial candidates on these policies, since ratings are constructed based on legislative votes. An objective way to measure their

[21]Every governor in the sample has the faculty to veto

commitment to fiscal austerity is by identifying signers of the "Taxpayer Protection Pledge" promoted by the organization "Americans for Tax Reform". Signers, pledge to oppose increases in income taxes for individuals and businesses.

I divide my sample into two groups: a first group of state elections with a pledge signer candidate, and a second group composed of all other state elections. Eight Republican candidates signed the pledge before election, while no Democratic candidate signed it[22]. The first two columns of Table 13 show the average effect for each group. The last two columns use a dummy variable for the second group to determine whether the difference between groups is statistically significant. The first row of Table 13 shows larger effects for signers compared to Table 4 results. The second row show lower effects for non-signers. Although positive, the difference between groups is not statistically significant.

Table 13: Effect by anti-tax commitment

	Levels			Difference	
Republican Share (Pledge Signers)	-60.52*** (21.58)	-67.84*** (20.62)	Republican Share	-61.51*** (21.26)	-68.57*** (20.34)
Republican Share (Non Pledge Signers)	-34.69** (14.84)	-29.43 (19.65)	Dummy Non Signers	22.73 (24.83)	28.25 (26.32)
Controls:					
U.S. Economy	Yes	Yes		Yes	Yes
U.S. CDS	Yes	Yes		Yes	Yes
CDX indices	Yes	Yes		Yes	Yes
ITRX indices		Yes			Yes
OECD CDS		Yes			Yes
State Fixed Effects	Yes	Yes		Yes	Yes
Month Fixed Effects	Yes	Yes		Yes	Yes
N	877/827	877/827		924	924
R-sq	0.32/0.33	0.39/0.41		0.33	0.39

[22]Source: "Americans for Tax Reform"

8 Conclusion

In this paper I present a new study of the effect that Republican and Democratic parties have on state risk of default. Republicans and Democrats have historically taken different approaches to fiscal policies that do not have an obvious effect on credit risk. The former pairs spending cuts with tax cuts while the latter matches tax increases with more spending. In order to find the net effect of both policies I consider the response of the default risk to changes in poll projections during gubernatorial election races. If an increase in the probability of victory translates into a decrease in credit spreads, the political party is good news for debtholders. I use Credit Default swaps data to obtain a measure of states default risk. I construct a unique poll database to obtain election predictions for gubernatorial elections between 2008 and 2012.

A difference in difference panel regression analysis shows that, ex-ante, the Republican Party is good news for debtholders. On average, prospects of Republican administrations reduce credit spreads by around six percent, more than half of CDS standard deviation during election race. I show evidence to support the following conjectures: The effect is stronger when candidates commit to fiscal austerity and for highly contested gubernatorial elections. I do not find evidence that debtholders have a preference for divided governments.

Estimates of the effect of political parties are not affected by the inclusion of more control variables and are statistically significant at 99% for every specification. Robustness checks that use an alternative independent variable also show evidence that Republican governors have a significant positive effect on default risk. An event-study presents additional evidence on the direction of this effect.

I obtain superior estimates of the effect of party affiliation using an alternative and innovative approach in political economics. This method uses high frequency financial data and a common assumption in the literature: that high frequency poll intentions are exogenous to high frequency economic variables. A panel adaptation of placebo regression analysis shows that polls cause changes in CDS spreads and that it is not a spurious correlation.

This paper focuses on political party control of the Executive branch by the two main U.S. political parties. Natural extensions are to employ this framework to compare conservative and liberal parties in Europe and other regions, and to explore the effect of legislative branch party control on default risk.

References

[1] Alesina, A. (1988). "Credibility and policy convergence in a two-party system with rational voters." The American Economic Review 78(4): 796-805.

[2] Alesina, Alberto F., and Jeffrey D. Sachs. "Political parties and the business cycle in the United States, 1948-1984." (1986).

[3] Alesina, A. and H. Rosenthal (1996). "A theory of divided government." Econometrica: Journal of the Econometric Society: 1311-1341.

[4] Alt, J. E. and D. D. Lassen (2006). "Fiscal transparency, political parties, and debt in OECD countries." European Economic Review 50(6): 1403-1439.

[5] Alt, J. E. and R. C. Lowry (1994). "Divided government, fiscal institutions, and budget deficits: Evidence from the states." American Political Science Review: 811-828.

[6] Ang, A., R. C. Green, et al. (2013). Advance Refundings of Municipal Bonds, National Bureau of Economic Research.

[7] Ang, A. and F. A. Longstaff (2013). "Systemic sovereign credit risk: Lessons from the US and Europe." Journal of Monetary Economics.

[8] Berg, J., R. Forsythe, et al. (2008). "Results from a dozen years of election futures markets research." Handbook of experimental economics results 1: 742-751.

[9] Berg, J. E., F. D. Nelson, et al. (2008). "Prediction market accuracy in the long run." International Journal of Forecasting 24(2): 285-300.

[10] Besley, T. and A. Case (1995). "Does electoral accountability affect economic policy choices? Evidence from gubernatorial term limits." The Quarterly Journal of Economics 110(3): 769-798.

[11] Besley, T. and A. Case (2003). "Political institutions and policy choices: evidence from the United States." Journal of Economic Literature 41(1): 7-73.

[12] Blais, A., D. Blake, et al. (1993). "Do parties make a difference? Parties and the size of government in liberal democracies." American Journal of Political Science: 40-62.

[13] Brauninger, T. (2005). "A partisan model of government expenditure." Public Choice 125(3-4): 409-429.

[14] Calvert, R. L. (1985). "Robustness of the multidimensional voting model: Candidate motivations, uncertainty, and convergence." American Journal of Political Science: 69-95.

[15] Caplan, B. (2001). "Has Leviathan been bound? A theory of imperfectly constrained government with evidence from the states." Southern Economic Journal: 825-847.

[16] Cestau, D., R. C. Green, et al. (2013). "Tax-subsidized underpricing: The market for build america bonds." Journal of Monetary Economics.

[17] Clark, W. R. and M. Hallerberg (2000). "Mobile capital, domestic institutions, and electorally induced monetary and fiscal policy." American Political Science Review: 323-346.

[18] Comiskey, M. (1993). "Electoral competition and the growth of public spending in 13 industrial democracies, 1950 to 1983." Comparative Political Studies 26(3): 350-374.

[19] Cusack, T. R. (1997). "Partisan politics and public finance: Changes in public spending in the industrialized democracies, 19551989." Public Choice 91(3-4): 375-395.

[20] Dye, T. R. (1984). "Party and Policy in the States." Journal of Politics 46(4): 1097-1116.

[21] Duffie, Darrell. "Credit swap valuation." Financial Analysts Journal (1999): 73-87.

[22] Ferreira, F. and J. Gyourko (2009). "Do political parties matter? Evidence from US cities." The Quarterly Journal of Economics 124(1): 399-422.

[23] Franzese Jr, R. J. (2000). Electoral and partisan manipulation of public debt in developed democracies, 195690. Institutions, politics and fiscal policy, Springer: 61-83.

[24] Fredriksson, P. G., L. Wang, et al. (2009). "Partisan Governors and Economic Policy."

[25] Gurkaynak, R. S., B. Sack, et al. (2007). "The US Treasury yield curve: 1961 to the present." Journal of Monetary Economics 54(8): 2291-2304.

[26] Garand, J. C. (1988). "Explaining government growth in the US states." The American Political Science Review: 837-849.

[27] Gilligan, T. W. and J. G. Matsusaka (1995). "Deviations from constituent interests: The role of legislative structure and political parties in the states." Economic Inquiry 33(3): 383-401.

[28] Green, R. C. (1993). "A simple model of the taxable and tax-exempt yield curves." Review of Financial Studies 6(2): 233-264.

[29] Green, R. C., B. Hollifield, et al. (2007). "Dealer intermediation and price behavior in the aftermarket for new bond issues." Journal of Financial Economics 86(3): 643-682.

[30] Green, R. C., B. Hollifield, et al. (2007). "Financial intermediation and the costs of trading in an opaque market." Review of Financial Studies 20(2): 275-314.

[31] Grogan, C. M. (1994). "Political-economic factors influencing state Medicaid policy." Political Research Quarterly 47(3): 589-622.

[32] Herron, M. C., J. Lavin, et al. (1999). "Measurement of political effects in the United States economy: a study of the 1992 presidential election." Economics & Politics 11(1): 51-81.

[33] Hibbs, D. A. (1977). "Political parties and macroeconomic policy." American Political Science Review 71(04): 1467-1487.

[34] Hibbs Jr, D. A. (1987). VThe American Political Economy: Macro'economics and Electoral PoliticsV Cambridge, MA: Harvard University Press.

[35] Knight, B. (2006). "Are policy platforms capitalized into equity prices? Evidence from the Bush/Gore 2000 presidential election." Journal of Public Economics 90(4): 751-773.

[36] Knight, B. G. (2000). "Supermajority voting requirements for tax increases: evidence from the states." Journal of Public Economics 76(1): 41-67.

[37] Lee, D. S. (2008). "Randomized experiments from non-random selection in US House elections." Journal of Econometrics 142(2): 675-697.

[38] Lee, D. S., E. Moretti, et al. (2004). "Do voters affect or elect policies? Evidence from the US House." The Quarterly Journal of Economics 119(3): 807-859.

[39] Longstaff, F. A. (2011). "Municipal debt and marginal tax rates: Is there a tax premium in asset prices?" The Journal of Finance 66(3): 721-751.

[40] Mattozzi, A. (2010). "POLICY UNCERTAINTY, ELECTORAL SECURITIES, AND REDISTRIBUTION*." International Economic Review 51(1): 45-71.

[41] Oatley, T. (1999). "How constraining is capital mobility? The partisan hypothesis in an open economy." American Journal of Political Science: 1003-1027.

[42] Poole, K. T. and H. Rosenthal (1984). "The polarization of American politics." The Journal of Politics 46(04): 1061-1079.

[43] Poole, K. T. and H. Rosenthal (1984). "US Presidential Elections 1968-80: A Spatial Analysis." American Journal of Political Science: 282-312.

[44] Poole, K. T. and H. Rosenthal (1991). "Patterns of congressional voting." American Journal of Political Science: 228-278.

[45] Potrafke, N. (2009). "Did globalization restrict partisan politics? An empirical evaluation of social expenditures in a panel of OECD countries." Public Choice 140(1-2): 105-124.

[46] Reed, W. R. (2006). "Democrats, Republicans, and taxes: Evidence that political parties matter." Journal of Public Economics 90(4): 725-750.

[47] Roemer, J. E. (1994). "The strategic role of party ideology when voters are uncertain about how the economy works." American Political Science Review: 327-335.

[48] Rogers, D. L. and J. H. Rogers (2000). "Political competition and state government size: Do tighter elections produce looser budgets?" Public Choice 105(1-2): 1-21.

[49] Rose, R. (1985). Public employment in Western nations, Cambridge University Press.

[50] Roubini, N. and J. D. Sachs (1989). "Political and economic determinants of budget deficits in the industrial democracies." European Economic Review 33(5): 903-933.

[51] Santa-Clara, P. and Valkanov, R. (2003), The Presidential Puzzle: Political Cycles and the Stock Market. The Journal of Finance, 58: 1841-1872. doi: 10.1111/1540-6261.00590

[52] Snowberg, E., J. Wolfers, et al. (2006). Party influence in congress and the economy, National Bureau of Economic Research.

[53] Tufte, E. R. (1980). Political control of the economy, Princeton University Press.

[54] Von Beyme, K. (1985). "The Role of the State and the Growth of Government." International Political Science Review 6(1): 11-34.

[55] Winters, R. (1976). "Party control and policy change." American Journal of Political Science: 597-636.

[56] Wittman, D. (1977). "Candidates with policy preferences: A dynamic model." Journal of Economic Theory 14(1): 180-189.

[57] Wittman, D. (1990). "Spatial strategies when candidates have policy preferences." Advances in the spatial theory of voting: 66-98.

A An application for the Federal Government

In this appendix I show an application for the presidential election 2012, Obama Vs. Romney. I use Intrade prediction market data, to get Romney's probability to win the presidential election and Obama's probability to win the election. Figure 1 plots the evolution of CDS credit spreads and Romney's probability of winning the presidential election in 2012. Table 10 presents estimate results using daily data and 10 year CDS spreads.

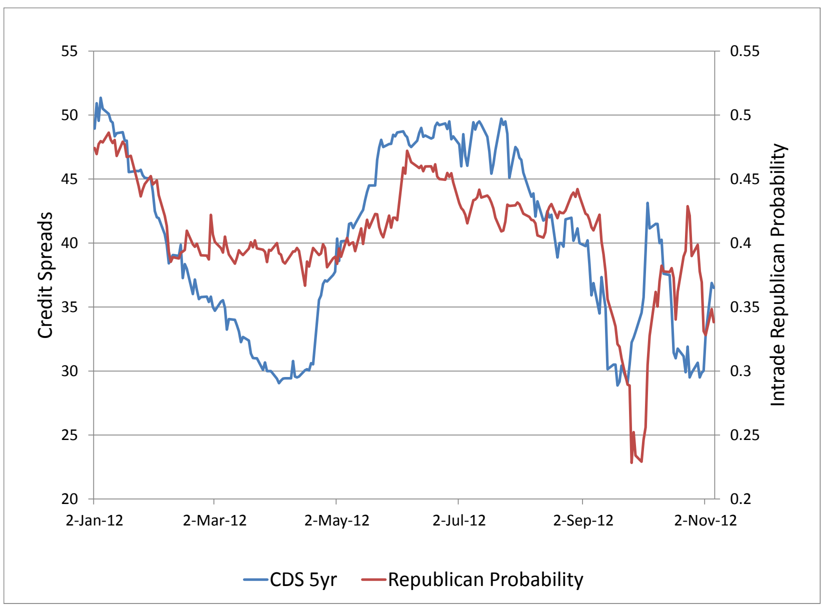


Figure 1: Credit Spreads Vs. Romney - Presidential Election 2012

Column 1 of table 10 shows that an increase in the probability of a Romney victory was bad news for sovereign debtholders in 2012 election. It is positive and statistically significant at 99% confidence. Columns two, three and four also present positive numbers with statistical significance ranging 95% and 99% of confidence. Panel D of table 3 analyze the effect of an Obama victory on credit spreads. Columns one to four show negative values with statistical significance of 99% confidence. The results are consistent with those obtained in panel R. They indicate that a democrat victory (Obama) was good news for debtholders. The coefficients offer a direct interpretation of its values. They measure the change in credit spreads from a one unit change in the probability of victory, i.e., the dichotomous difference from going from Obama to Romney and vice versa.

Table 14: 10 Years CDS. Presidential Election 2012, 1 year before election

	Panel R Intrade Republican Victory				Panel D Intrade Democrat Victory			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Prob	69.48***	68.45***	57.09**	37.13	-69.15***	-71.38***	-74.01***	-87.24***
	(0.006)	(0.007)	(0.019)	(0.118)	(0.006)	(0.005)	(0.004)	(0.003)
Lag Prob		-26.16	-20.07	-0.33		17.08	11.98	-14.77
		(0.233)	(0.350)	(0.988)		(0.517)	(0.656)	(0.554)
Oct Prob			35.96***	119.3**			8.107***	24.54
			(0.000)	(0.014)			(0.000)	(0.272)
Lag Oct Prob			-9.604	-26.46			-0.67	16.69
			(0.172)	(0.616)			(0.578)	(0.660)
Congress				Yes				Yes
Mkt Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
S.C. Prob	.24	.24	.20	.13	-.22	-.23	-.24	-.28
N	196	196	196	196	196	196	196	196
R-sq	0.227	0.234	0.241	0.261	0.158	0.161	0.175	0.229

Robust p-values between parentheses
* p<0.10, ** p<0.05, *** p<0.01

Chapter 2:

Admitting Students to Selective Education Programs: Merit, Profiling, and Affirmative Action.

Admitting Students to Selective Education Programs: Merit, Profiling, and Affirmative Action*

Dario Cestau

Carnegie Mellon University

Dennis Epple

Carnegie Mellon University

Holger Sieg

University of Pennsylvania

April 23, 2014

*We would like to thank Derek Neal and two referees as well as Peter Arcidiacono, Pat Bayer, Flavio Cunha, Glen Ellison, John Engberg, Hanming Fang, Eric Hanushek, Joe Hotz, Sunny Ladd, Kjell Salvanes, Petra Todd, and seminar participants at numerous workshops and seminars for comments and suggestions. Financial support for this research is provided by the Institute of Education Sciences. Email: D. Cestau: dcestau@andrew.cmu.edu, D. Epple: epple@cmu.edu, H. Sieg: holgers@econ.upenn.edu.

Abstract

For decades, colleges and universities have struggled to increase participation of minority and disadvantaged students. Urban school districts confront a parallel challenge; minority and disadvantaged students are underrepresented in selective programs that use merit-based admission. We analyze optimal school district policy and develop an econometric framework providing a unified treatment of affirmative action and profiling. Implementing the model for a central-city district, we find profiling by race and income, affirmative action for low-income students, and no affirmative action with respect to race. Counterfactual analysis reveals that these policies achieve 80% of African American enrollment that could be attained by race-based affirmative action.

KEYWORDS: Gifted and talented education, profiling, affirmative action, merit-based admission, equilibrium analysis, estimation.

1 Introduction

For decades, colleges and universities have grappled with the challenge of increasing participation of minority and disadvantaged students. Differential admissions criteria designed to increase participation of minority and disadvantaged students inevitably collide with concerns for fairness toward those who are displaced. Courts, legislatures, and electorates have weighed in to define the acceptable limits of affirmative action in higher education.[1] While the spotlight has largely focused on higher education, school districts in large metropolitan areas confront the same challenge; minority and disadvantaged students are underrepresented in selective programs for gifted and talented students.[2] Efforts to increase participation of minority and disadvantaged students in these programs are no less controversial than their higher-education counterparts.[3]

School districts nationwide make a large investment in providing programs to serve gifted and talented students.[4] The National Association of Gifted Children estimates that there are approximately three million academically gifted children in grades K-12 in the U.S - almost seven percent of the student population. As a proportion of

[1]The two most recent affirmative action rulings by the US Supreme Court are on Fisher v University of Texas (June 24, 2013) and Schuette v. Coalition to Defend Affirmative Action (April 22, 2014). We return to these in our policy analysis.

[2]To cite a recent example, on September 27, 2012, a complaint was filed with the U.S. Department of education claiming that the admissions policies of eight elite public high schools in New York City violate the 1964 Civil Rights Act, causing extreme under-representation of black and Hispanic students. (Baker, 2012).

[3]The New York Times dubbed the director of the city's gifted and talented program a "lightning rod for fury" when she announced changes to the admissions policies for the city's gifted programs (NYT, March 22, 2006). Approximately, two years later, the times reported: "When New York City set a uniform threshold for admission to public school gifted programs last fall, it was a crucial step in a prolonged effort to equalize access to programs that critics complained were dominated by white middle-class children whose parents knew how to navigate the system." The new policy has also met with intense criticism, and the director of the city's gifted and talented program has concluded: "We implemented the eligibility criteria, it didn't shake out that way and now we have to take another look at it." (NYT, June 19, 2008)

[4]The empirical evidence on the impact of gifted and talented programs on achievement is discussed in Abdulkadiroglu, Angrist, & Pathak (2011).

the student population, this is comparable to the percentage of high school students admitted to selective colleges and universities.[5] Most large public school districts in the U.S. operate selective programs to serve gifted and talented students.[6] Admission to these programs is competitive and merit-based. In this paper we investigate access a gifted and talented program of a mid-sized urban school district. The referral and admission processes used for the gifted program in the district we study are widely used elsewhere. To provide just a few examples here, the state of Florida prescribes procedures that very closely parallel those used by our district, as does the Los Angeles Unified District, the second largest school district in the country, and the Atlanta district.

School districts must promulgate admission policies that establish eligibility criteria. A district must also set up referral or testing policies that determine which students are evaluated as potential candidates for the program. Finally, the district must determine whether criteria will include consideration of disadvantaged or minority status. We document the divergence between the demographics of the district's student body relative to the demographics of those participating in the district's gifted program. We delineate the constraints under which the district operates and the policy tools available to the district. We then study properties of optimal district policy. We formulate and estimate a model to characterize the district's use of profiling and affirmative action policies to enhance participation of minority and disadvantaged students while respecting the merit-based guidelines promulgated by the state government. We believe this to be the first econometric model providing a

[5]Reardon, et. al. (2012) identify the 171 most selective colleges and universities and report that approximately 7% of high school students attend one of these selective institutions.

[6]International Baccalaureate programs also use stringent admission procedures. The most highly ranked public high schools in New York, Boston, and Philadelphia all use merit-based admission procedures.

unified treatment of affirmative action[7] and profiling[8] in an equilibrium framework that permits counterfactual analysis of policy.

The goal of gifted and talented programs is to provide education to help highly able students reach their potential.[9] Districts typically utilize achievement or intelligence tests in their admissions decisions; our district uses an IQ test. Ability is difficult to observe and, more importantly, non-verifiable, while IQ can be observed and verified by parents and teachers. Testing is costly. For the district we study, an IQ test is administered orally on a one-to-one basis between a district psychologist and each student considered for gifted admission. Testing is also stressful for parents and students, and can be viewed as an unwelcome burden in cases where students have a low probability of gaining admission to the selective program. Hence, a referral procedure is needed to determine which students take the IQ test.

In designing a referral procedure, the district must make a judgment of the extent to which the benefits of the program vary with observed student characteristics such as income or race. This judgment may reflect an assessment of potential gains that

[7]Becker (1957) introduced the analysis of taste based discrimination into economics. Phelps (1972) and Arrow (1973) developed a theory of statistical discrimination. The effects of affirmative action in employment have been studied by Lundberg (1991), Coate & Loury (1993), and Moro & Norman (2003, 2004). Chung (2000) considers the relationship between role models and affirmative action. Affirmative action in higher education is considered by Chan & Eyster (2003) , Loury, Fryer, & Yuret (2008), and Epple, Romano, & Sieg (2006, 2008). Arcidiacono (2005) estimates a structural model to determine how affirmative action in admission and aid policies affects future earnings. Hickman (2010a, 2010b) develops and estimates a structural model of college admissions and compares the effects of alternative admissions policies on incentives for academic achievement, the racial achievement gap, and the racial college enrollment gap. Long (2007) provides a valuable summary of the legal status of affirmative action and a review of the evidence regarding the effects of affirmative action.

[8]Knowles, Persico, & Todd (2001) stimulated a large body of work on profiling in law enforcement. In higher education, research on profiling includes Loury et al. (2008), Epple et al. (2008), and Epple, Romano, Sarpca, and Sieg (2012). State policies mandating admission of a specified fraction of each high school to state universities, motivated in part to achieve racial balance through profiling, are studied by Long (2004a, 2004b) and Cullen, Long, & Reback (2012).

[9]Neal & Johnson (1996) show that black-white differences in premarket skill do account for a significant portion of the black- white earnings gap in the early 1990's.

students experience from participating in the program. Since ability is not fully observed, there is uncertainty about these gains. Design of a referral procedure then entails balancing the potential benefits against the costs of testing. We characterize the optimal referral and admission policy for the district. We characterize the way in which referral and admission policies depend on the weights that the district assigns to the potential gains to different students. One key property of our model is that optimal policies are cut-off strategies.

A student who has, in some way, experienced hardship may underperform on achievement tests relative to his or her capability. This motivates use of profiling in referral and admission.[10] Profiling can be implemented by use of empirically grounded differences in distributions of achievement, IQ, and ability for different demographic groups. Two primary arguments are generally advanced in support of affirmative action. One is that, by facilitating enrollment of a critical mass of minority students, affirmative action reduces the isolation otherwise experienced by minority students. The other is that the potential for interactions among diverse peers benefits both majority and majority students.[11] Affirmative action may be manifest in adoption of different referral or admission criteria for different demographic groups when such differences in criteria are not attributable to profiling. We study optimal referral and admission policies when a district is permitted to employ both profiling and affirmative action.

To gain additional insights into the quantitative properties of our model and demonstrate its practical relevance, we parametrize the model and develop a Maximum Likelihood Estimator that exploits the fact that important variables are either

[10] The state guidelines require that districts consider "intervening factors" such as "gender or race bias, or socio/cultural deprivation" that may "mask gifted abilities."

[11] These arguments are detailed in the University of Michigan brief in Grutter v. Bollinger: http://www.vpcomm.umich.edu/admissions/legal/grutter/UM-Grutter.pdf

latent (ability), partially latent (IQ scores), or measured with error (prior achievement). We estimate the parameters of the district's objective function from observed test scores, gifted referrals, and admissions by race and economic disadvantage (eligibility status for free or reduced-price lunch) of different demographic groups Our empirical analysis is based on a sample of elementary school students that entered first grade in the academic year 2004-05. We find that our model fits the data well and that estimated referral and admission thresholds are consistent with policies articulated by the district. We find that the district adopts profiling with respect to both race and subsidized lunch status. The district also uses affirmative action in admission with respect to subsidized lunch status. We do not find any evidence that the district engages in affirmative action based on race.

A large proportion of African American students are eligible for free or reduced lunch (FRL). As a consequence, our estimated model implies that applying the FRL admission policies to all African American students, regardless of FRL status, would have a modest effect on increasing African American enrollment in the gifted program. Hence, in contrast to higher education[12], we find that profiling that exploits FRL status can have a quite substantial effect in increasing minority student participation. Our model also implies that eliminating preferential policies for FRL students and standardizing referral and admission decisions at the level of non-FRL students would reduce the size of the gifted program by up to 50 percent. We thus conclude that adopting strict merit based referral and admission policies, would significantly alter the size and composition of the program.

The rest of the paper is organized as follows. Section 2 develops the theoretical model of optimal referral and admission for a merit-based selective program. Section 3

[12] In his review of affirmative action in colleges and universities, Long (2007) concludes that "...correlates of race are unlikely to substitute successfully for the consideration of race itself."

discusses the implications of affirmative action and profiling within our model. Section 4 introduces a parameterization of the model and develops a Maximum Likelihood Estimator. Section 5 discusses our data set. Section 6 reports the empirical results of this study. Section 7 discusses the policy implications that can be drawn from this analysis. Section 8 offers some conclusions.

2 A Model of Merit-Based Referral and Admission

We consider the decision problem of a public school district that operates a selective educational program. Admission is competitive and merit-based. Ability is inherently difficult to observe and cannot be verified. Admission criteria are, therefore, based on prior academic achievement and a standardized aptitude or IQ test. Testing is costly. Hence, not all students are referred for testing. We model the choice of policy for referral of students for testing and the choice of policy for admission to the selective program.

There is a continuum of students that differ by ability, b. Let q denote the student's score on an IQ test and a performance on a prior achievement test. Let $f(a, b, q)$ be the joint density of achievement, academic ability and IQ score in the population. Students may also differ by discrete characteristics such as race or low income status. We first present the analysis for a single discrete type and then extend to consideration of more than one such type.

Assumption 1 *The density $f(a, b, q)$ is continuous on its support $(\underline{a}, \bar{a}) \times (0, \bar{b}) \times (0, \bar{q})$.*[13]

[13]For notational simplicity, we assume in the following that the upper bounds are infinity and the lower bound for achievement is negative infinity.

Gifted programs are motivated by the objective of targeting education to high-ability students to help them develop their capabilities. The value or value-added that the district attaches to having a student of type b participate in the selective program is denoted by $v(b)$.

Assumption 2

a) The value function $v(b)$ is continuous and differentiable. Moreover, it is monotonically increasing in b, i.e. $v_b(b) \geq 0$.

b) The cost of testing a student a student is constant and equal to c.[14]

The district initially observes a and chooses a referral policy denoted by $\alpha(a)$. The testing procedure provides the student IQ score. a and q are both informative of students ability, hence the district grants access to the gifted program based on this information.

The districts optimally chooses a referral policy and a admission policy. We make the following assumptions about the information revelation and hence the nature of the admission and referral process.

Assumption 3

a) The decision rule that determines who is referred for testing is a function of prior achievement:

$$0 \leq \alpha(a) \leq 1 \tag{1}$$

b) The decision rule that determines admission is a function of the IQ score and prior

[14]The IQ test employed by the district is an interactive test administered by a psychologist to an individual student. State law mandates that gifted status be determined by a certified district psychologist. Cost is typically cited as the reason for limiting the number of students who are tested. (See NYT, June 19, 2008)

achievement:

$$0 \leq \beta(a,q) \leq 1 \tag{2}$$

The district forms beliefs in a Bayesian way and chooses the optimal referral and admission policies to maximize the expected difference between benefits and costs. The objective function of the district is then given by:

$$\begin{aligned} &\int_{-\infty}^{\infty}\int_{0}^{\infty}\int_{0}^{\infty} \alpha(a)\ \beta(a,q)\ v(b)\ f(a,b,q)\ da\ db\ dq \\ &- \int_{-\infty}^{\infty}\int_{0}^{\infty}\int_{0}^{\infty} \alpha(a)\ c\ \ f(a,b,q)\ da\ db\ dq \end{aligned} \tag{3}$$

The first term captures the expected benefits of the program. The second term captures the costs due to IQ testing.[15]

We can solve the decision problem above using standard techniques of variational calculus. It is optimal for the district to use a cut-off strategy for both referral and admission policies.

Proposition 1 *The optimal solution of our model can be characterized by thresholds* $\bar{a}$ *and* $\bar{q}(a)$.

All proofs are given in the Appendix.

As shown in the proof of Proposition 1, the district will only admit a student to the gifted program, if he or she performs sufficiently well on the IQ score, i.e. if the

[15] An alternative approach is to model the behavior of the district as wishing to allocate limited space in the gifted program based on fairness or efficiency considerations. These ideas are explored in Abdulkadiroglu (2005) and Kojima (2012).

expected value added to the program is non-negative:

$$V(a,q) = \int_0^\infty \int_0^\infty v(b)\ f(b|a,q)\ db\ \geq\ 0 \tag{4}$$

The district is indifferent if the equation above holds with equality. This condition implies a threshold function $\bar{q}(a)$ such that a student is admitted to the program if and only $q \geq \bar{q}(a)$.

Similarly, the optimality condition for referral for IQ testing can be written as:

$$W(a) = \int_{\bar{q}(a)}^\infty \int_0^\infty v(b)\ f(b,q|a)\ db\ dq \quad \geq \quad c \tag{5}$$

The first term measures the benefits of the students that are referred. The second term captures the costs associated with testing. The district is indifferent if the equation above holds with equality. This condition then defines a threshold function $\bar{a}$ such that a student is referred to the program if and only $a \geq \bar{a}$.

Note that participation in the gifted programs is voluntary. Students that are admitted to the gifted program by the district can opt out of the gifted program and attend regular classes. Based on our conversations with members of the school district we know that the take-up rate among students is very high, easily exceeding 90 percent. As a consequence, while we do not observe take-up decisions by individual students, we feel quite confident that the take-up rate is sufficiently high as to make this issue moot. For applications where acceptance of admission is less routine, it would not be difficult to extend the model and the estimation procedure to allow for voluntary participation decisions as long as the research observes admission and attendance decisions.

3 Affirmative Action and Profiling

Students from disadvantaged backgrounds tend to be underrepresented in selective programs. The district can use profiling to take into account factors (e.g., hardship) that may cause underperformance relative to ability on achievement tests. Being a minority student or a student with FRL status are observable indicators that may be correlated with having experienced hardship. Another potential strategy to reduce underrepresentation in selective programs is affirmative action. Either profiling or affirmative action can give rise to different criteria for such students and can potentially be adopted in either admission or referral policies. In this section, we undertake analysis to provide an economic foundation for understanding and distinguishing affirmative action and profiling in referral and admission policies.

To study the implications of profiling and affirmative action, we extend our model to consider two discrete types. Let m denote a student of a disadvantaged or minority type and M denote the advantaged or majority type.[16]

Let $f_j(a,b,q)$ $j = m, M$ now denote the density of the each type and let $v_j(b)$ denote the value that the district assigns to type j student given ability b.

3.1 Affirmative Action

Affirmative action arises in our model if the district deviates from an equal treatment policy and gives preferential treatment to minority students. We formalize this concept of affirmative action as follows.

Definition 1 *Preference based affirmative action arises if the school districts assigns*

[16] In our empirical application we focus on African Americans and students that are eligible for free or reduced lunch.

a higher value to students of type m than students of type M, i.e if

$$v_m(b) \geq v_M(b) \tag{6}$$

where equation (6) holds with strict the inequality for a set of b with positive measure.

Affirmative action then leads to lower referral and admission thresholds for minority students. We can formalize this result by considering the case in which there are no differences in the underlying distribution of types.

Proposition 2 *If $f_m(a,b,q) = f_M(a,b,q)$, then preference based affirmative action implies that referral and admission thresholds are lower for minorities, i.e. that $\bar{a}_m \leq \bar{a}_M$ and $\bar{q}_m(a) \leq \bar{q}_M(a)$.*

The intuition for this result is straight-forward. A student is admitted to the gifted program if the expected benefit conditional on prior achievement and IQ is positive. Preference based affirmative action raises the benefits for minority students and as a consequence lowers the admission threshold. Lower admission thresholds also imply lower referral thresholds.

Thus far, we have cast the discussion in terms of differences between minority and majority students where minority status can be defined based on race (black vs non-black) or income (subsidized lunch vs regular lunch). However, it is also possible that heterogeneity in treatment arises for a variety of other reasons. For example, there may be systematic differences in treatment of students from different neighborhoods. For example, the district may use the gifted program as a tool to retain students from higher income neighborhoods in district schools. Even if not formally adopted by the district, similar variation may arise if, for example, the district defers to principals to some degree in determining referral thresholds. In our empirical analysis below, we extend our model to test whether there is evidence of such policy variation across school types.

3.2 Profiling

In practice, we observe significant differences in the distribution of observed outcomes between minority and majority students which suggests that $f_m(a,b,q) \neq f_M(a,b,q)$. As a consequence the district may want to adopt different referral and admission thresholds even if it does not engage in preference based affirmation action. The intuition for this result is a follows. If minorities perform, on average, worse on achievement and intelligence test, then the district should take these differences into consideration when making referral decision. We show later in the paper that some minority students perform better on IQ tests than majority students for some levels of school achievement, ($F_M(q|a) \geq F_m(q|a)$ for some values of a). This in turn implies that for some levels of achievement minorities students are likely to have higher ability than majority students. In that case the district may want to engage in profiling.

Definition 2 *The district engages in profiling if it takes the differences in the conditional distribution of ability into consideration when making referral and admission decisions.*

To isolate the effects of profiling in referral, we have the following result:

Proposition 3 *If $v_m(b) = v_M(b)$ and if $F_M(b|a,q) \geq F_m(b|a,q)$, then the district gives preferential treatment to minority students in admission decisions, i.e. $\bar{q}_m(a) \leq \bar{q}_M(a)$.*

The intuition is as follows. Suppose, among students of a given ability, minority students perform worse than majority students on prior achievement tests scores.

As a consequence, for given prior achievement, minority students are likely to be more able than majority students. Since the value function of the district depends on ability, these students have higher expected benefits from gifted admission. This then translates into a lower admission threshold for minority than for majority students.

4 Estimation

Let R denote a discrete random variable that is equal to one if the student is referred for testing and zero otherwise. Let A denote a discrete random variable that is equal to one if the student is accepted into the selective program.

Assumption 4 *Consider a random sample of students.*

1. *We observe R for all students in the sample.*
2. *For students, who are referred for testing ($R = 1$), we observe q and A.*
3. *We observe achievement with error, denoted by $\tilde{a}$.*
4. *We observe race and subsidized lunch status.*
5. *We do not observe b for any student.*

We assume that (a, b, q) are jointly normally distributed for each type j. Measured IQ equals ability plus a normally distributed error that is independent of (a, q)

$$q = b \ + \ \epsilon_q \tag{7}$$

The equation above implies that $E_j[b] = E_j[q]$ and $Var_j(b) < Var_j(q)$. Errors for achievement and ability are classical measurement errors and depend on the observed type j.

The district's value function for type j is given by:

$$v_j(b) \;=\; \zeta_{0j} \;+\; \zeta_{1j}\, b \tag{8}$$

In our application, we consider four types using race and FRL status to define types.

Given this parametrization, we can obtain a closed form solution of the admission policy and a simple characterization of the referral policy. As shown in Appendix D, we have the following result:

Proposition 4 *Given the parametrization above, the optimal admission policy is given by the linear function* $\bar{q}_j(a) \;=\; \tau_{0j} \;-\; \tau_{1j}\, a$ *where:*

$$\begin{aligned} \tau_{0j} &= \frac{-\frac{\zeta_{0j}}{\zeta_{1j}} \;-\; \mu_{b_j} \;+\; A_j\, \mu_{a_j} \;+\; Q_j\, \mu_{q_j}}{Q_j} \\ \tau_{1j} &= \frac{A_j}{Q_j} \end{aligned} \tag{9}$$

where A_j *and* Q_j *are known functions of the parameters of the model. A student of type* (j, a, q) *is admitted to the program if and only if* $q \geq \bar{q}_j(a)$.

The proof of Proposition 4 also implies that $W_j(a)$ can be computed fairly efficiently by univariate integration. As a consequence there exists a unique $\bar{a}_j$, which is the solution to the equation $W_j(a) = c$ and can be efficiently computed.

With measurement error in achievement, the probability of observing $R = 0$ conditional on j and $\tilde{a}$ is given by:

$$Pr\{R = 0\;|j, \tilde{a}\} = \int_0^{\bar{a}_j} \; f_j(a|\tilde{a})\; da \tag{10}$$

Similarly, the probability of observing $R = 1$ and $A = 1$ is:

$$Pr\{R = 1, A = 1|\ j, q, \tilde{a}\} = \int_{\bar{a}}^{\infty} 1\{q \geq \bar{q}_j(a)\} \;\; f_j(a|q, \tilde{a})\ da \tag{11}$$

The probability of observing $R = 1$ and $A = 0$ is:

$$Pr\{R = 1, A = 0|\ j, q, \tilde{a}\} = \int_{\bar{a}}^{\infty} 1\{q < \bar{q}_j(a)\} \;\; f_j(a|q, \tilde{a})\ da \tag{12}$$

The likelihood for a single observation is then given by:

$$\begin{aligned} L \;\; = \;\; & [Pr\{R = 0|\ j, \tilde{a}\}\ f_j(\tilde{a})]^{(1-R)} \\ & [Pr\{R = 1, A = 1|\ j, q, \tilde{a}\}\ f_j(\tilde{a}, q)]^{R_q\ A} \\ & [Pr\{R = 1, A = 0|\ j, q, \tilde{a}\}\ f_j(\tilde{a}, q)]^{R\ (1-A)} \end{aligned} \tag{13}$$

The likelihood for a sample of N students is then a straightforward product of the above for the N observations. We can, therefore, consistently estimate the parameters of the model using a Maximum Likelihood estimator. Appendix E shows that our estimator works well in a Monte Carlo study.

Finally, we offer a number of observations regarding the identification of the parameters of the model. Consider the model with one type.[17] The parameters of the joint distribution of prior achievement and IQ are identified of the observed empirical distributions. While we do not observe IQ scores for the students that are not referred for testing, our structural model endogenously generates the relevant selection rule.

Identification of the distribution of latent ability is achieved as follows. First, the mean of the ability distribution is identified from (7). The variance of σ_b is then identified from the admission probabilities, which can be seen as follows. Consider

[17] The identification argument easily extends to the case of multiple types.

one limiting case in which $\sigma_b = \sigma_q$. In that case, (7) implies $b = q$, and the admission probability is a (deterministic) function of q. Note that the variance of the IQ score is equal to the variance of ability plus the variance of the IQ error. The higher the variance of the error the smaller is the covariance between IQ and ability which then affects the correlation between the admission variable and IQ. In the other limiting case, admission is only a function of prior achievement since the IQ score is uninformative. The higher the variance in the IQ error the higher is the weight that we place on prior achievement in the admission decision.

The covariance between ability and achievement is identified from the observed covariance between IQ and prior achievement. The covariance between prior achievement and ability is also identified by the conditional admission probabilities. An increase in σ_{ab} implies an increase in the weight that is assigned to a in the admission decision and, thus, increases the observed correlation between admissions and prior achievement.

Consider equation (9) and note that A_j and Q_j are known functions of parameters of the joint distribution of achievement, IQ and ability. We can treat these as known. Proposition 4 then implies that the admission locus only depends on the term $-\frac{\zeta_0}{\zeta_1}$. Hence the ratio of the coefficients of the value function are identified from the admission probabilities. The referral threshold depends, however, on the level of the expected net benefits. We normalize testing costs, c, to be equal to \$100.[18] The levels of the coefficients of the value function are thus identified from the normalization of testing costs and the observed conditional referral probabilities.

The error variance of prior achievement is identified from the degree of misclassification observed in the data. If the error variance were zero, our model should per-

[18] It is not possible to separately identify the costs of testing from the intercept of the district's utility function. As a consequence, we normalize the costs of testing based on our knowledge of the testing process and the known market prices of private testing.

fectly explain the observed referral decisions once we condition on the observed prior achievement score. Differences between the model's predictions and the observed outcomes are only due to measurement error in prior achievement. We, therefore, rely on measurement error in prior achievement to generate a well-defined likelihood function.[19]

5 Data

Our application focuses on a selective gifted program that is operated by a mid-sized urban district that prefers to stay anonymous. The district operates a Gifted Center that serves students in elementary and middle school. Gifted students in grades 1 through 8 participate in a one-day-per-week program at a designated location away from the student's home school. Students participate in programs designed to enhance creative problem solving and leadership skills and are offered specially designed instruction in math, science, literature, and a variety of other fields.

The district adheres to state regulations concerning gifted students and services. The state regulations outline a multifaceted approach used to identify whether a student is gifted and whether gifted education is needed. The state requires gifted status to be determined by a certified district psychologist. A mentally gifted student who is not on free and reduced lunch is defined as someone with an IQ score of at least 130. The regulation specifies that a student with IQ score below 130 may be admitted "... when other educational criteria in the *profile* of the person strongly indicates gifted ability (emphasis added)." The state guidelines provide for consideration of factors

[19] Our estimating approach is also related to work on latent factor models by Carneiro, Hansen, & Heckman (2003) and Cunha & Heckman (2008) who also treat ability as a latent variable. As in our model, identification can be achieved without imposing strong linearity or normality assumption as discussed in Schenach (2004) and Cunha, Heckman, & Schenach (2010).

that may ”mask” giftedness including “. . . gender or race bias, or socio/cultural deprivation. . . ” Thus, the state gives district discretion to employ profiling and affirmative action in referral and admission policies. Of course, the district is also subject to the U.S. constitution, which has been interpreted by the Supreme Court as requiring that affirmative action by race be a last resort in achieving diversity objectives.

The IQ tests that the district uses are the most widely used tests in the U.S. The tests differ primarily by student age. For the age range that we are studying, the district uses the fourth edition of the Wechsler Intelligence Scale for Children (WISC4). The district administers this examination using standard WISC4 protocol, one-on-one, to a student by a psychologist trained in IQ testing. This test is designed for elementary school children and is administered orally so that IQ assessment is not heavily driven by the degree to which the student has developed reading and writing skills sufficient to comprehend and complete a written exam. The WISC4 has roughly 15 subtests designed to elicit a range of cognitive capabilities; each subtest in turn is comprised of a set of questions. The tester asks questions in a prescribed order and scores student responses.

The district codes electronically the students IQ score, but not the scores on the subtests or the name of the psychologist administering the test. Thanks to excellent cooperation from the district, we were permitted to send a team of PhD students to the district offices for a period of many days to pull each student’s paper file, scan the student’s IQ test report into a file, and, from these, code the sub-scores. We also coded the name of the psychologist who tested each student. We find some evidence of testers “giving the benefit of the doubt” to students who are very close to the threshold. For example, for most psychologists, the proportion of students scoring at the threshold is somewhat larger than the proportion scoring one or two points below the threshold. This appears to be modest in magnitude (on the order of 2 to

3 points), and is not systematically linked to student observables.

Another concern with matching our model into the data is that our model does not allow for private testing. Moreover, private testers may employ more lenient standards than public testers giving some opportunity to parents to buy access to the gifted program. Fortunately, our data set contains detailed information about the identify of person that administered the IQ test allowing us to differentiate between private and public testers. We find that there are only 12 students in the sample for whom we observe private test scores. These are all students from higher income families and do not quality for free or reduced lunch. For all 12 students we observe one private test score. All of them are admitted to the program based on the private test. 11 out of the 12 students were also tested by a public psychologist. The average private test is 130.4 compared to 117.5 for the publicly administered test. Note that the average IQ of an accepted regular lunch student from a public test is 131.9. Of course, there may be a larger number of students who are privately tested, but it is reasonable to assume that we only observe private test scores when they are "above the bar." While there is some evidence that private testers are more lenient than public testers, we conclude that the number of students that are admitted into the gifted program based on private test score is sufficiently small that it does not raise serious doubts about our modeling and estimation approach.

Most referrals and admissions for the gifted program occur during elementary school in grades 1-4. We focus on this population in this paper. The sample consists of the cohort of students that were in 1^{st} grade in 2004/05. We follow these students until the end of $4th$ grade.

We start with a sample of 3,306 students. We keep those students who were at the school district during the first four grades of school. This reduces our sample by roughly one half, to 1,592 students. We drop a number of observations without lunch status data and that changed their lunch status over the four school grades. We eliminate 9 observations without achievement data. We drop 12 observations for students who were admitted based on IQ scores provided by private testers after failing to gain admission by the IQ test administered by the district.[20] Finally, we eliminate duplicate observations that correspond to students who were referred more than once. The final sample size is 1,265 observations.

Pre-referral achievement scores are constructed based on a variety of scores. We observe the Oral Reading Fluency (ORF) test. We standardize ORF scores for every period in every grade and compute the average score for each student. In addition we have access to Pennsylvania System of School Assessment (PSSA) scores that measure reading and math skills in 3rd grade. Finally, we use an average GPA score. Our prior achievement measure as a weighted average of these types of scores.[21]

As previously noted, we use eligibility for free or reduced-price lunch as a measure of economic disadvantage for students. We also create a poverty indicator for each school which ranges between 0 and 2. To construct this variable, students eligible for free lunch are coded as 2, students eligible for reduced-price lunch are coded as 1, and regular lunch students are coded as zero. The school income indicator is then the school average of this variable over a ten year period. We use this variable to classify schools into two discrete categories, high- and low-income schools. We test whether there are significant differences in referral and admission decisions for each type. Table 1 reports descriptive statistics for the full sample as well as the subsamples by FRL status and race.

We find, as with many other urban districts, that there is a high proportion of poor

[20] These 12 individuals are all non-FRL students. The average of their scores on the district test was 117.5 and the average on the privately administered tests was 130.4.

[21] The weights are determined by regressing a referral indicator on the available achievement scores. We experimented with alternative weightings of achievement scores and found that our results are robust.

Table 1: Sample Statistics by FRL status and Race

		Lunch status		Race	
	All types	Non-FRL	FRL	Non Black	Black
Sample size	1265	299	966	539	726
FRL	0.76			0.54	0.93
Black	0.57	0.17	0.70		
Achievement	0.00	0.81	-0.25	0.42	-0.31
Income school	0.50	0.93	0.37	0.75	0.32
Fraction Referred	0.16	0.43	0.08	0.26	0.09
Fraction Gifted	0.09	0.28	0.03	0.17	0.03
Ratio Gifted/Referred	0.56	0.66	0.38	0.66	0.34

students and the majority of students are black. Achievement and school income are negatively correlated with FRL status and race. Table 1 also shows that 92 percent of all African American students in our sample are FRL eligible while only 51 percent of non-black students are FRL eligible.

Table 2 reports the demographics of the sample by referral and gifted status. We find that FRL and black students tend to be underrepresented among referred and gifted students. Not surprisingly, students referred for testing have higher achievement scores than students who are not referred. Similarly gifted students have significantly higher achievement and IQ scores than non-gifted students.

Table 2: Statistics for Non Referred, Referred and Gifted Students

			Referred students only	
	Non referred	Referred	Non Gifted	Gifted
FRL	0.84	0.37	0.52	0.25
Black	0.62	0.32	0.47	0.19
Achievement	-0.26	1.37	1.02	1.64
IQ			110	129

To set the stage for our econometric analysis, we provide some additional evidence regarding the referral process. We break the support of the achievement distribution into eight intervals. Table 3 reports the frequency of referral for each bin conditional on FRL status and race. We find that referral probabilities are monotonically increasing in the our observed measure of prior achievement. There are some pronounced differences in referrals probabilities by FRL status. There are much smaller difference in referral probabilities by race.

Table 3: Ratios of Referred Students by Achievement

	Regular		FRL	
Achievement	total	% Referred	total	% Referred
<0	67	0.00	598	0.01
0≤ <0.33	28	0.07	120	0.06
0.33≤ <0.66	30	0.17	93	0.13
0.66≤ <1.00	38	0.42	73	0.25
1.00≤ <1.33	44	0.50	49	0.27
1.33≤ <1.66	35	0.80	21	0.57
1.66≤ <2.00	24	0.96	9	0.67
2≤	33	0.97	3	1.00
	Non Black		Black	
Achievement	total	% Referred	total	% Referred
<0	190	0.00	475	0.01
0≤ <0.33	62	0.03	86	0.08
0.33≤ <0.66	64	0.06	59	0.22
0.66≤ <1.00	53	0.34	58	0.28
1.00≤ <1.33	68	0.41	25	0.28
1.33≤ <1.66	40	0.73	16	0.69
1.66≤ <2.00	29	0.86	4	1.00
2≤	33	0.97	3	1.00

To illustrate some of the properties of the admission process, we break the support of the IO distribution into six intervals. Table 4 reports the frequency of IQ for each bin conditional of FRL status and race.

Table 4: Ratios of Gifted Students by IQ

	Regular Lunch		FRL	
IQ score	total	% Admitted	total	% Admitted
<110	12	0.00	27	0.00
110≤ <115	10	0.00	11	0.00
115≤ <120	13	0.15	19	0.58
120≤ <125	18	0.44	9	1.00
125≤ <130	24	1.00	4	1.00
130≤	51	1.00	4	1.00
	Non Black		Black	
IQ score	total	% Admitted	total	% Admitted
<110	15	0.00	24	0.00
110≤ <115	12	0.00	9	0.00
115≤ <120	18	0.33	14	0.50
120≤ <125	18	0.56	9	0.78
125≤ <130	24	1.00	4	1.00
130≤	51	1.00	4	1.00

The upper panel of Table 4 breaks down the sample by free or reduced lunch status, the lower panel by race. We find that admission probabilities are increasing in IQ scores. For students who are not eligible for FRL, the probability of acceptance is equal to one if the IQ score is larger than 125. It drops to 44 percent if the IQ score is between 120 and 125. It is 18 percent for students with scores between 115 and 120. For students eligible for FRL, the admission probability is one for all students with scores larger than 120. It drops to 58 percent for students with IQ between 115 and 120. It is zero for student with IQ score below 115. We view these findings as evidence suggesting that students on FRL face lower admission thresholds.

Finally, we consider admission by race in the lower panel of Table 4. We find that there are some small difference in the admission probabilities between black and non-black students in the interval between 115 and 125. These differences are much smaller that the differences we observe by FRL status.

6 Empirical Results

We begin by estimating our model allowing the fullest possible variation in policies across types with respect to both profiling and affirmative action. We designate this Model I. We then investigate the extent to which policy variation across types can be restricted without significantly reducing the fit of the model. It is useful to begin by summarizing the set of parameters to be estimated in Model I. The utility function of the district is linear and has two parameters for each type. With affirmative action in admission varying across four types (race and FRL), there are then 8 parameters in the utility function. With full profiling, the distribution parameters differ across types. For each type, we have means and covariances for ability and achievement. This yields five parameters per type for a total of 20 parameters. In addition, there are eight measurement error variances for achievement and IQ by type. Hence, the most general specification, Model I, has 36 parameters.

Model specifications that impose common parameters in the underlying distributions of ability, IQ and achievement are clearly rejected by the data. Hence, we consider restrictions regarding the use of affirmative action while retaining type specific distributions of ability, achievement and measurement and, thus, allow for profiling.We consider four models that vary in the use of affirmative action. Model I allows for 8 different district utility parameters, thereby allowing for affirmative action by FRL status and race. Model II allows for affirmative action by FRL status, but not race. This imposes 4 restrictions on the parameter space. Model III imposes the additional restriction that the slope parameters of the district utility function are the same, implying equal marginal benefit to ability across types. Finally, Model

IV does not allow for affirmative action imposing 6 restrictions on the parameters of the utility function. Table 5 summarizes these model specifications and reports the relevant log-likelihood statistics. Since these models are nested, we can use standard likelihood ratio tests to evaluate the different specifications. Table 5 suggests that Model III is our preferred model specification. Model III imposes five restrictions relative to Model I. The difference in likelihood function values between these two models implies a chi-squared statistic of 5.6 and p=.25. Model IV is very strongly rejected ($p < .001$) relative to either Model I or Model III.

Table 5: Model Summary

Model	Profiling		Affirmative Action Intercept		Affirmative Action Slope		Likelihood
	FRL	Race	FRL	Race	FRL	Race	
I	Yes	Yes	Yes	Yes	Yes	Yes	-2653.9
II	Yes	Yes	Yes	No	Yes	No	-2655.8
III	Yes	Yes	Yes	No	No	No	-2656.7
IV	Yes	Yes	No	No	No	No	-2689.2

Our finding that Model III is our preferred model carries the implication that the district does not apply different criteria in admission decisions for black relative to non-black students. Profiling with respect to race enhances prediction of performance by race, but does not imply preferential treatment by race. Hence, we conclude that the district provides no preferential treatment by race. By contrast, the district does give preferential treatment with respect to FRL status.

Next we discuss the estimation results in further detail. Table 6 reports goodness of fit statistics conditional on race for the four models. Table 6 shows that the first

Table 6: Goodness of Fit

	Data	Model I	Model II	Model III	Model IV
	Non Black Students				
Not Referred	74.4	75.1	75.2	75.1	75.3
Referred not Admitted	8.7	9.3	8.9	9.5	8.3
Referred Admitted	16.9	15.6	15.9	15.5	16.4
IQ Referred not Admitted	112.3	113.5	113.3	113.7	111.3
IQ Referred and Admitted	131.0	131.8	131.7	131.7	130.0
	Black Students				
Not Referred	91.2	91.3	91.4	91.3	91.7
Referred not Admitted	5.8	6.0	5.9	5.9	5.4
Referred Admitted	3.0	2.7	2.8	2.8	2.9
IQ Referred not Admitted	108.3	108.8	108.6	108.6	109.4
IQ Referred and Admitted	123.0	123.6	124.4	124.2	128.0
	Non-FRL Lunch Students				
Not Referred	57.2	58.6	58.6	58.3	59.4
Referred not Admitted	14.4	15.7	15.4	16.4	13.8
Referred Admitted	28.4	25.7	26.0	25.3	26.8
IQ Referred not Admitted	113.0	114.4	114.3	114.5	110.8
IQ Referred and Admitted	131.9	132.8	132.8	132.9	130.4
	FRL Students				
Not Referred	92.3	92.4	92.5	92.5	92.5
Referred not Admitted	4.8	4.8	4.6	4.7	4.4
Referred Admitted	2.9	2.8	2.9	2.9	3.1
IQ Referred not Admitted	108.0	108.2	107.9	107.9	110.1
IQ Referred and Admitted	122.1	122.8	123.2	122.8	127.4
	All Students				
Not Referred	84.0	84.4	84.5	84.4	84.7
Referred not Admitted	7.0	7.4	7.2	7.4	6.7
Referred Admitted	8.9	8.2	8.4	8.2	8.7
IQ Referred not Admitted	110.4	111.3	111.1	111.3	110.4
IQ Referred and Admitted	129.5	130.1	130.3	130.2	129.6
Black in Admitted	19.5	19.1	19.0	19.4	0.2
FRL in Admitted	24.8	25.9	26.5	26.7	0.3

three models all fit the data almost equally well. By contrast, the fit of the Model IV is significantly worse for black and FRL students.

We now turn to the parameter estimates. For computational reasons we renormalize the utility function as $v_j(b) = \zeta_{0j} + \zeta_{1j}\ (b - 100) = \zeta_{1j}\ (\Delta_j + b)$, where $\Delta_j = (\zeta_{0j} - 100 * \zeta_{1j})/\zeta_{1j}$. To save some space, Table 7 reports the estimates and estimated standard errors only for our preferred specification, Model III. Beginning with mean achievement estimates in the top panel, we see that students on FRL have lower achievement than their non-FRL, same-race counterparts. We see as well that non-black students have higher achievement than black students. The standard deviations of achievement exhibit some variation across race and FRL, with variance of non-white, non-FRL students being moderately higher than for the other three student types.

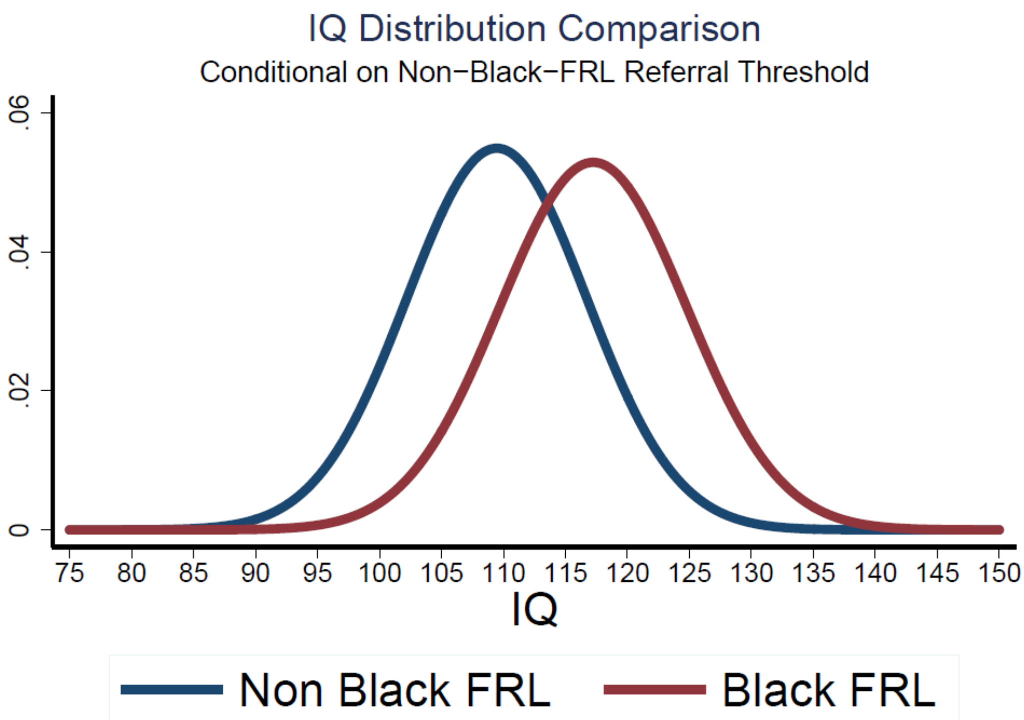
Our estimates of the parameters of ability are shown in the second panel. The means of ability exhibit a striking contrast to the means of achievement. Mean ability estimates for those on FRL are virtually the same for black and non-black students. For students not on FRL, non-black students have a higher mean than black students, but the relative difference is substantially smaller than for achievement. The contrast between the variation in means of achievement by race and FRL to the variation in means of ability by race and FRL provides insight into our finding of significant scope for profiling by race and FRL. We see, in addition, that the estimated correlation between achievement and ability for FRL students is lower for black (.84) than non-black (.93) students. The standard deviations of ability are modestly higher for non-FRL than for FRL students. Table 7 also reveals, not surprisingly, that there is substantial error in measurement of both ability and achievement.

We provide in Figure 1 an illustration of how differences in distributions by type provide scope for profiling. Figure 1 plots the conditional distributions of IQ scores for

Table 7: Parameter Estimates

achievement distribution	$\mu_{a,rw}$	0.88 (0.07)	$\mu_{a,fw}$	0.03 (0.05)
	$\mu_{a,rb}$	0.47 (0.12)	$\mu_{a,fb}$	-0.37 (0.03)
	$\sigma_{at,rw}$	0.98 (0.04)	$\sigma_{at,fw}$	0.88 (0.04)
	$\sigma_{at,rb}$	0.87 (0.09)	$\sigma_{at,fb}$	0.83 (0.02)
	$\sigma_{e,rw}$	0.49 (0.03)	$\sigma_{e,fw}$	0.45 (0.06)
	$\sigma_{e,rb}$	0.48 (0.07)	$\sigma_{e,fb}$	0.54 (0.04)
ability distribution	$\mu_{b,rw}$	113.68 (2.31)	$\mu_{b,fw}$	91.78 (6.07)
	$\mu_{b,rb}$	102.58 (5.72)	$\mu_{b,fb}$	84.52 (5.23)
	$\sigma_{b,rw}$	15.98 (1.85)	$\sigma_{b,fw}$	14.07 (3.40)
	$\sigma_{b,rb}$	14.45 (4.09)	$\sigma_{b,fb}$	15.93 (2.33)
	$\sigma_{q,rw}$	5.95 (0.65)	$\sigma_{q,fw}$	5.00 (1.44)
	$\sigma_{q,rb}$	2.33 (2.09)	$\sigma_{q,fb}$	3.92 (1.03)
correlations	$\rho_{ab,rw}$	0.93 (0.01)	$\rho_{ab,fw}$	0.93 (0.04)
	$\rho_{ab,rb}$	0.84 (0.11)	$\rho_{ab,fb}$	0.91 (0.04)
value function	$\Delta_{,rw}$	-122.13 (0.69)	$\Delta_{,rb}$	-122.13 (0.51)
	$\Delta_{,fw}$	-115.20 (0.69)	$\Delta_{,fb}$	-115.20 (0.51)
	$\zeta_{1,rw}$	896 (413)	$\zeta_{1,rb}$	896 (413)
	$\zeta_{1,fw}$	896 (413)	$\zeta_{1,fb}$	896 (413)

Figure 1:



IQ Distribution Comparison
Conditional on Non−Black−FRL Referral Threshold
0
.02
.04
.06
75 80 85 90 95 100 105 110 115 120 125 130 135 140 145 150
IQ
Non Black FRL
Black FRL

a given value of achievement for black and non-black FRL students. The achievement level is set to the 1.05 which is the referral threshold for non-black FRL students. Our findings illustrate that, among relatively high-achieving FRL students, the IQ scores of black students are higher than that of non-black students, or stated differently, black students have lower achievement relative to ability than non-black students.

From the last panel of Table 7, we see that the intercept of the district utility function for FRL students is higher than for non-FRL students, implying affirmative action in favor of FRL students. The difference in intercepts is quite substantial, equivalent to adding approximately .45 standard deviation to ability of FRL students. We established above in our comparison of Model IV to Model III that this difference is highly significant.

Finally, we investigate whether different schools use different referral and admission policies. To conduct this analysis, we consider two sets of schools: high-income and low-income schools. We then allow different utility functions for the two school types, and hence different referral and admission thresholds. There are very few non-FRL students in poor schools, too few to permit meaningful testing of whether low-income schools treat non-FRL students differently than high-income schools. We, therefore, investigate whether preferences for gifted participation of FRL students differ between low-income and high-income schools by allowing the intercept of the district utility function to differ between high- and low-income schools. We obtain a difference in intercepts of 1.5, with low-income schools provide somewhat higher affirmative-action benefits to FRL students than high-income schools. This difference is relatively modest, equivalent to adding .1 standard deviation to ability of FRL student in low-income schools, and not significant by conventional standards (p=.09). Hence, we conclude that there is limited, if any, heterogeneity in treatment of FRL students between high- and low-income schools.

7 Policy Analysis

In 2003, the U.S. Supreme Court considered two affirmative action cases against the University of Michigan, naming then-president Lee Bollinger, as defendant. One, Grutter v. Bollinger (2003), challenged the admission policy of the law school. The other, Gratz v. Bollinger (2003), challenged the undergraduate admission policy. The court upheld the former, seeing it as narrowly construed and employing race only as a possible favorable factor, while striking down the latter as being too closely akin to a quota system. The court decision with respect to Michigan's undergraduate admission policy prompted many colleges and universities to change their admission policies to reduce or eliminate race as a distinct admission criterion.

The Gratz v. Bollinger decision prompted research investigating the extent to which race-blind policies, coupled with selection employing correlates of race, might preserve racial diversity in higher education.[22] Here we use race-blind to mean that criteria for student admission do not vary by race. These analyses suggest that, at the college level, banning affirmative action while permitting of use of correlates of race results in a very significant reduction in minority attendance, especially at top-tier institutions.

It is likely that the Gratz v. Bollinger decision also induced many school districts, possibly including the one that we study, to adopt race-blind referral and admission policies. Subsequent Supreme Court decisions directly focused on K-12 education reinforce the need for school districts develop race-blind policies. In 2007, the Court decided two cases involving K-12 education, one in Seattle (Parents Involved in Community Schools v. Seattle School District No. 1) and the other in Louisville (Meredith v. Jefferson County Board of Education). In those cases, the Court invalidated the

[22]See, for example, Chan & Eyster (2003), Epple et al. (2008) and Loury et al. (2008).

districts' racial desegregation plans, ruling that students could not be assigned to schools solely to achieve racial balance. Not surprisingly, the Court was closely divided. Casting the pivotal vote, Justice Kennedy sided with the conservative wing of the Court to form a majority to ban use of race in determining school assignments. In his opinion, however, Justice Kennedy pointedly left the door open to use of correlates of race to achieve race-conscious objectives.[23] In 2006, voters in Michigan approved a ban on affirmative, which in turn was upheld in the April 2014 Supreme Court decision in Schuette v Coalition to Defend Affirmative Action. That decision has prompted increased focus on use of socioeconomic factors as an alternative to use of race in fostering affirmative action objectives.[24]

The following important policy question then arises: How closely can race-conscious but race-blind policies achieve the objectives of racial integration of gifted K-12 academic programs? Our model permits us to provide an analysis of the effect on racial diversity of affirmative action based on race. We can compare this approach to the effect of affirmative action based on a correlate of race—FRL status. We, therefore, consider two policy alternatives. Policy 1 employs affirmative action with respect to both FRL and race. This is accomplished by extending the admission threshold for FRL students to all African American Students. Policy 2 eliminates affirmative action entirely.

Our model predicts that adoption of Policy 1 would increase gifted enrollment of African American students in this cohort from 19.4 to 23.3 percent. Under Pol-

[23] In Fisher v University of Texas (June 24, 2013), the Supreme Court strongly emphasized that placing any explicit weight on race in admissions should be a last resort, but, in its remand to the lower court, the Supreme Court again seemed to accept use of correlates of race to achieve race-conscious objectives: "The reviewing court must ultimately be satisfied that no workable race-neutral alternatives would produce the educational benefits of diversity."

[24] The President of the American Association of State Colleges and Universities commented that "Most of us have already started to look at other variables than race, especially first-generation students, and low-income students." (NYT, 4/22/2014).

Table 8: Policy Analysis

	Data	Model III	Policy 1	Policy 2
	Non Black Students			
Not Referred	74.4	75.1	75.1	78.2
Referred not Admitted	8.7	9.5	9.4	7.9
Referred Admitted	16.9	15.5	15.4	13.9
IQ Referred not Admitted	112.3	113.7	113.7	115.4
IQ Referred and Admitted	131.0	131.7	131.6	132.9
	Black Students			
Not Referred	91.2	91.3	89.8	95.5
Referred not Admitted	5.8	5.9	6.8	3.3
Referred Admitted	3.0	2.8	3.5	1.3
IQ Referred not Admitted	108.3	108.6	106.6	113.0
IQ Referred and Admitted	123.0	124.2	123.1	129.3
	Non-FRL Lunch Students			
Not Referred	57.2	58.3	54.6	58.3
Referred not Admitted	14.4	16.4	18.4	16.4
Referred Admitted	28.4	25.3	27.0	25.3
IQ Referred not Admitted	113.0	114.5	112.1	114.5
IQ Referred and Admitted	131.9	132.9	132.0	132.9
	FRL Students			
Not Referred	92.3	92.5	92.5	97.3
Referred not Admitted	4.8	4.7	4.7	1.8
Referred Admitted	2.9	2.9	2.9	0.9
IQ Referred not Admitted	108.0	107.9	107.9	114.7
IQ Referred and Admitted	122.1	122.8	122.8	129.2
	All Students			
Not Referred	84.0	84.4	83.5	88.1
Referred not Admitted	7.0	7.4	7.9	5.2
Referred Admitted	8.9	8.2	8.6	6.6
IQ Referred not Admitted	110.4	111.3	110.2	114.5
IQ Referred and Admitted	129.5	130.2	129.7	132.6
Black in Admitted	19.5	19.4	23.3	10.9
FRL in Admitted	24.8	26.7	25.4	10.0

icy 2, enrollment of African American students would drop to 10.9 percent. Thus, eliminating affirmative action would reduce African American student enrollment by approximately 50% relative to a policy of affirmative action for African American students. Eliminating affirmative action for African American students but retaining affirmative action for FRL students reduces African American student enrollment by 20%. Thus, affirmative action with respect to FRL is extremely important in preserving enrollment of African American students in the district's gifted program.

8 Conclusions

U.S. educational institutions at all levels invest extensive resources in providing advanced programs to help high-ability students reach their potential. The challenge that arises in deciding admission to these programs is that ability is unobserved. Obtaining measures of ability that are not affected by family resources and vagaries of circumstance has proven to be extraordinarily challenging. Despite extensive research and design efforts, measures of ability such as IQ exhibit systematic variation across demographic groups. Educational institutions confront the challenge of using available evidence to assess suitability of students for admission to selective programs, distinguishing ability from influences attributable to family resources and circumstance. While not uncontroversial, differential treatment based on income has gained a considerable measure of acceptance. Race-based criteria have proven to be much more controversial.

The Supreme Court has endorsed a holistic approach, one that considers a broad set of factors in decisions about the admission of a student to selective programs and institutions, i.e., the Court encourages profiling. Going somewhat farther in his decision for the majority in Gratz versus Bollinger, Justice Kennedy explicitly declined to reject use of non-race factors to achieve race-conscious ends, a view he reaffirmed in a concurring opinion in the Seattle and Louisville cases. It is natural to wonder whether it is possible to formalize the distinctions being made by the court, and, if so, whether it is possible to test empirically whether a policy does or does not conform to court guidelines. In this light, we have pursued four objectives in this paper. One objective is to develop a framework sufficiently broad not only to encompasses merit, profiling, and affirmative action, but also to permit statistical testing of the roles that these factors play in practice. A second is to demonstrate, via econometric analysis, the practical relevance of this framework in application. A third is to extend the analysis of these policy issues to an important domain that has been understudied, selective programs for high-ability students in primary and secondary education. A fourth objective is to make a substantive contribution to policy by statistical testing of the extent of use of profiling and affirmative action in an operational setting, and by counterfactuals that investigate the consequences, either of curtailing or extending, the use of profiling or affirmative action.

We believe our application succeeds in these objectives. Of particular interest are the findings of our counterfactual analysis of race-based affirmative action. Long (2007), in his review of the evidence with respect to race-based affirmative action in colleges and universities, concludes, and we agree, that: "The pressures—and in some cases, legal requirements—to replace affirmative action are very real. All the same, the stark evidence of the inefficacy of replacement programs remains." By contrast, for the central school district we study, we find that profiling by race and income, coupled with affirmative action by income, can achieve 80 percent of the level of African American enrollment that could be achieved by race-based affirmative action.

Two questions naturally arise: Why the sharp contrast to findings with respect to elimination of race-based affirmative action in our setting as compared to findings for colleges and universities? To what extent is it likely to generalize to other central city school districts? The answer to the first question is that middle- and upper-income households with children tend to locate in suburban school districts, leaving relatively low-income households in the central city districts. African American households are disproportionately represented among low-income households. Hence, in central city districts, policies that courts have thus far found admissible (profiling by race and income and affirmative action by income) can go a long way toward achieving outcomes that would be achieved by adding race-based affirmative action to the district's decision criteria. The answer to the second question is that our findings are indeed likely to generalize to other central city school districts; the propensity of middle- and high-income households with children to locate in suburban districts is a phenomenon observed in every large metropolitan area in the United States.

The methods developed in this paper offer much scope for future research. Our framework may also be applicable in other settings in which costly screening procedures are used with the goal of increasing minority participation.[25] For example, costly search for qualified minority applicants as an alternative or complement to affirmative action is quite common in post-secondary and post-graduate education (e.g., minority recruitment fairs hosted by colleges). Minority job fairs often require participating candidates to submit a specified set of materials that are then reviewed by participating employers as they decide which candidates will be invited for interviews.

[25] We are grateful to an anonymous referee for pointing out the potential relevance of our framework in these domains.

References

Abdulkadiroglu, A. (2005). College admission with affirmative action. *International Journal of Game Theory*, *33*, 535–49.

Abdulkadiroglu, A., Angrist, J., & Pathak, P. (2011). The elite illusion: achievement effects at boston and new york exam schools. NBER Working Paper 17264.

Anwar, S. & Fang, H. (2006). An alternative test for racial prejudice in motor vehicle searches: theory and evidence. *American Economic Review*, *96 (1)*, 127–151.

Arcidiacono, P. (2005). Affirmative action in higher education: how do admission and financial aid rules affect future earnings?. *Econometrica*, *73 (5)*, 1477–1524.

Arrow, K. (1973). The theory of discrimination. In Ashenfelter, O. (Ed.), *Discrimination in Labor Markets*. Princeton University Press.

Baker, A. (2012). Charges of bias in admission test policy at eight elite public high schools. *New York Times*, *Sept 22*.

Becker, G. (1957). *The Economics of Discrimination*. Chicago University Press.

Carneiro, P., Hansen, K., & Heckman, J. (2003). Estimating distributions of treatment effects with an application to the returns of schooling and measurement of the effects of uncertainty on college choice. *International Economic Review*, *44(2)*, 361–422.

Chan, J. & Eyster, E. (2003). Does banning affirmative action harm college quality?. *American Economic Review*, *93 (3)*, 858–873.

Chung, K. (2000). Role models and arguments for affirmative action. *American Economic Review*, *90*, 640–48.

Coate, S. & Loury, G. (1993). Will affirmative action policies eliminate negative stereotypes?. *American Economic Review*, *83*, 1220–40.

Court, U. S. (2003a). Gratz and versus and bollinger, 539 u.s. 244..

Court, U. S. (2003b). Grutter versus bollinger, 539 u.s. 306..

Court, U. S. (2003c). Meredith v. jefferson county board of education, 548, us 938,..

Court, U. S. (2007). Parents involved in community schools v. seattle school district no. 1, 551 u.s. 701..

Cullen, J., Long, M., & Reback, R. (2012). Jockeying for position: strategic high school choice under texas' top ten percent plan. *Journal of Public Economics*, *forthcoming.*

Cunha, F. & Heckman, J. (2008). Formulating, identifying and estimating the technology of cognitive and noncognitive skill formation. *Journal of Human Resources*, *43(4)*, 738–782.

Cunha, F., Heckman, J., & Schenach, S. (2010). Estimating the technology of cognitive and non-cognitive skill formation. *Econometrica*, *78 (3)*, 883–931.

Epple, D., Romano, R., & Sieg, H. (2006). Admission, tuition, and financial aid policies in the market for higher education. *Econometrica*, *74(4)*, 885–928.

Epple, D., Romano, R., & Sieg, H. (2008). Diversity and affirmative action in higher education. *Journal of Public Economic Theory*, *10 (4)*, 475–501.

Hickman, B. (2010a). Effort, race gaps, and affirmative action: a game-theortic analysis of college admissions. Working Paper.

Hickman, B. (2010b). Effort, race gaps, and affirmative action: a structural policy analysis of u.s. college admissions. Working Paper.

Knowles, J., Persico, N., & Todd, P. (2001). Racial bias in motor vehicle searches: theory and evidence. *Journal of Political Economy*, *109*, 203–229.

Kojima, F. (2012). School choice: impossibilities for afformative action. *Games and Economic Behavior*, *75*, 685–93.

Long, M. (2004a). College applications and the effect of affirmative action. *Journal of Econometrics*, *121*, 319–42.

Long, M. (2004b). Race and college admissions: an alternative to affirmative action. *Review of Economics and Statistics*, *86 (4)*, 1020–33.

Long, M. (2007). Affirmative action and its alternatives in public universities: what do we know?. *Public Administration Review*, *67 (2)*, 315–30.

Loury, G., Fryer, R., & Yuret, T. (2008). An economic analysis of color-blind affirmative action. *Journal of Law, Economics, and Organization*, *24(2)*, 319–55.

Lundberg, S. (1991). The enforcement of equal opportunity laws under imperfect information: affirmative action and alternatives. *The Quarterly Journal of Economics*, *106*(1), 309–26.

Moro, A. & Norman, P. (2003). Affirmative action in a competitive economy. *Journal of Public Economics*, *87*, 567–94.

Moro, A. & Norman, P. (2004). A general equilibrium model of statistical discrimination. *Journal of Economic Theory*, *114*, 1–30.

Neal, D. & Johnson, W. (1996). The role of pre-market factors in black-white wage differences. *Journal of Political Economy*, *104(5)*, 869–895.

Phelps, E. (1972). The statistical theory of racism and sexism. *American Economic Review*, *62*, 659–661.

Reardon, S., Baker, R., & Klasik, D. (2012). Race, income, and enrollment patterns in highly selective colleges, 1982-2004. Stanford University Working Paper.

Schenach, S. (2004). Estimation of nonlinear models with measurement error. *Econometrica*, *72 (1)*, 33–75.

A Proof of Proposition 1

Optimal admission for referral policies for IQ testing satisfies:

$$\alpha(a)\begin{pmatrix} =1 \\ \in[0,1] \\ =0 \end{pmatrix} \text{ as } \int_0^\infty\int_0^\infty \Big(\beta(a,q)\; v(b) \;-\; c\Big)\; f(b,q|a)\; db\; dq \begin{pmatrix} >0 \\ =0 \\ <0 \end{pmatrix} \tag{14}$$

Optimal admission satisfies:

$$\beta(a,q)\begin{pmatrix} =1 \\ \in[0,1] \\ =0 \end{pmatrix} \text{ as } \int v(b)\; f(b|a,q)\; db \begin{pmatrix} >0 \\ =0 \\ <0 \end{pmatrix} \tag{15}$$

Rearranging the FOC gives the result.

B Proof of Proposition 2

If $v_m(b) \geq v_M(b)$ and $f_m(a,b,q) = f_M(a,b,q) = f(a,b,q)$, then we have

$$\begin{aligned} V_m(a,q) &= \int_0^\infty\int_0^\infty v_m(b)\; f(b|a,q)\; db \\ &\geq \int_0^\infty\int_0^\infty v_M(b)\; f(b|a,q)\; db \\ &= V_M(a,q) \end{aligned} \tag{16}$$

and hence $\bar{q}_m(a) \leq \bar{q}_M(a)$. Similarly

$$\begin{aligned} W_m(a) &= \int_{\bar{q}_m(a)}^{\infty} \int_0^{\infty} v_m(b)\ f(b,q|a)\ db\ dq \\ &\geq \int_{\bar{q}_M(a)}^{\infty} \int_0^{\infty} v_m(b)\ f(b,q|a)\ db\ dq \\ &\geq \int_{\bar{q}_M(a)}^{\infty} \int_0^{\infty} v_M(b)\ f(b,q|a)\ db\ dq \\ &= W_M(a) \end{aligned} \tag{17}$$

which proves the result.

C Proof of Proposition 3

If $v_m(b) = v_M(b)$ and $F_m(b|a,q) \leq F_M(b|a,q)$, then we have

$$\begin{aligned} V_m(a,q) &= \int_0^{\infty} \int_0^{\infty} v(b)\ f_m(b|a,q)\ db \\ &\geq \int_0^{\infty} \int_0^{\infty} v(b)\ f_M(b|a,q)\ db \\ &= V_M(a,q) \end{aligned} \tag{18}$$

where the inequality follows from the fact that $v(b)$ is monotonically increasing in b. Hence $q_m(a) \leq q_M(a)$.

D Proof of Proposition 4

The expected value of admitting a student type (a,q) is given by:

$$V(a,q) \ = \ \int_0^{\infty} \int_0^{\infty} v(b)\ f(b|a,q)\ db \ = \ \zeta_0 + \zeta_1\ \mu_{b/a,q} \tag{19}$$

The covariance matrix is given by:

$$\Sigma_{b\;a\;q} \;=\; \begin{bmatrix} \sigma_b^2 & \sigma_{ab} & \sigma_b^2 \\ \sigma_{ab} & \sigma_a^2 & \sigma_{ab} \\ \sigma_b^2 & \sigma_{ab} & \sigma_b^2+\sigma_q^2 \end{bmatrix} \tag{20}$$

Hence the conditional expectation of b given (a,q) is given by the following equation:

$$\begin{aligned} \mu_{b/a,q} &= \mu_b + \begin{bmatrix} \sigma_{ab} & \sigma_b^2 \end{bmatrix} \begin{bmatrix} \sigma_a^2 & \sigma_{ab} \\ \sigma_{ab} & \sigma_b^2+\sigma_q^2 \end{bmatrix}^{-1} \begin{bmatrix} a - \mu_a \\ q - \mu_q \end{bmatrix} \\ &= \mu_b - (A\mu_a \;+\; Q\mu_q) \;+\; Aa \;+\; Qq \end{aligned} \tag{21}$$

where the constants A and Q are defined as:

$$\begin{aligned} A &= \frac{\sigma_{ab}(\sigma_b^2+\sigma_q^2) \;-\; \sigma_b^2\sigma_{ab}}{\sigma_a^2(\sigma_b^2+\sigma_q^2) \;-\; \sigma_{ab}^2} \\ Q &= \frac{-\sigma_{ab}^2 \;+\; \sigma_b^2\sigma_a^2}{\sigma_a^2(\sigma_b^2+\sigma_q^2) \;-\; \sigma_{ab}^2} \end{aligned} \tag{22}$$

The school is indifferent between admitting and not-admitting a student to the gifted program if the expected value added of the program is zero:

$$0 \;=\; V(a,q) \;=\; \zeta_0 + \zeta_1\,(\mu_b - (A\mu_a \;+\; Q\mu_q) \;+\; Aa \;+\; Qq) \tag{23}$$

Equation (23) defines a linear function $\bar{q}(a)$ which fully characterizes the admission policy $\beta(a,q)$:

$$\bar{q}(a) \;=\; \tau_0 \;-\; \tau_1\, a \tag{24}$$

where:

$$\tau_0 = \frac{-\frac{\zeta_0}{\zeta_1} - \mu_b + (A\mu_a + Q\mu_q)}{Q}$$
$$\tau_1 = \frac{A}{Q} \qquad (25)$$

Substituting the optimal admission rule into the expected value of referring a student type (a) for testing, we obtain:

$$W(a) = \int_0^{\infty} \int_{\bar{q}(a)}^{\infty} v(b)\ f(b,q|a)\ dq\ db \qquad (26)$$

Now $f(b,q|a) = f(b|a)\ f_{\varepsilon}(q-b)$. The mean and variance of the condition distribution of b given a are given by

$$\mu_{b/a} = \mu_b + \frac{\sigma_{ab}}{\sigma_a^2}(a - \mu_a) \qquad (27)$$

and

$$\sigma_{b/a}^2 = \sigma_b^2 - \frac{\sigma_{ab}^2}{\sigma_a^2} \qquad (28)$$

Hence, we have:

$$v(b)\ f(b,q/a) = (\zeta_0 + \zeta_1\ b)\ \frac{1}{\sqrt{2\pi}\sigma_c} e^{-\frac{(b-mu_c)^2}{2\sigma_c^2}} \frac{1}{\sqrt{2\pi}\sigma_q} e^{-\frac{(q-b)^2}{2\sigma_q^2}} \qquad (29)$$

Substituting (29) into (26), we obtain:

$$W(a) = \int_{-\infty}^{\infty} [1 - \Phi(\frac{\tau_0 - \tau_1 a - b}{\sigma_\epsilon})]\ (\zeta_0 + \zeta_1\ b)\ \frac{1}{\sqrt{2\pi}\sigma_c} e^{-\frac{(b-mu_c)^2}{2\sigma_c^2}}\ db \qquad (30)$$

$\bar{a}$ is then the unique solution to the equation $W(a) = c$. A student is referred if and only if $a \geq \bar{a}$. Letting the structural parameters of the underlying distribution of (a, b, q) and the parameters of the utility function to be functions of j then proves the result.

E A Monte Carlo Study

We conducted a Monte Carlo study to get some additional insights into the small sample properties of our ML estimator. We generated 50 samples with sizes equal to 5000 observations. Table 1 summarizes the findings of the Monte Carlo exercise. It reports the true parameter values under which the data were generated, the mean of the estimates, the standard deviation of the estimates and the sample mean of the estimated asymptotic standard errors.

Table 9: Monte Carlo Results

	True Value	Guess	Mean	Std Dev.	Asymp. Errors
μ_a	0.00	0.00	0.00	0.01	0.01
μ_b	100.00	105.00	99.97	1.09	0.99
σ_b	13.00	12.00	13.06	0.60	0.58
$\sigma_{IQ\ error}$	7.00	7.50	6.99	0.32	0.28
$\sigma_{\widetilde{a}\ error}$	0.35	0.40	0.35	0.01	0.01
$\sigma_{\widetilde{a}}$	1.00	1.00	1.00	0.01	0.01
$\rho_{a,b}$	0.65	0.70	0.65	0.03	0.03
$\Delta = \frac{\zeta_0 - 100 * \zeta_1}{\zeta_1}$	-116.67	-100.00	-116.68	0.23	0.24
(ζ_0)	(-5000)	(0.00)			
ζ_1	300	250	299.48	36.16	36.14

Simulations: 50. Simulations size: 7500

We find that the small sample bias of most parameters are small.

Chapter 3:

Tax-subsidized underpricing: The market for Build America Bonds

Journal of Monetary Economics 60 (2013) 593–608

Contents lists available at SciVerse ScienceDirect

Journal of Monetary Economics

journal homepage: www.elsevier.com/locate/jme

Tax-subsidized underpricing: The market for Build America Bonds

Dario Cestau [a], Richard C. Green [a,*], Norman Schürhoff [b,c,d]

[a] Tepper School of Business, Carnegie Mellon University, 5000 Forbes Avenue, Pittsburgh, PA 15213, United States
[b] Faculty of Business and Economics, University of Lausanne, 1015 Lausanne, Switzerland
[c] Swiss Finance Institute, Bd du Pont d'Arve 40, 1211 Geneva, Switzerland
[d] CEPR, 77 Bastwick St, London EC1V 3PZ, United Kingdom

ARTICLE INFO

Article history:
Received 30 January 2013
Received in revised form
8 April 2013
Accepted 23 April 2013
Available online 8 May 2013

ABSTRACT

Build America Bonds (BABs) were issued by municipalities for 20 months as a part of the 2009 fiscal package. Unlike traditional tax-exempt municipals, BABs are taxable to the holder, but the Treasury rebates 35% of the coupon to the issuer. The stated purpose was to provide municipalities access to a more liquid market including foreign, tax-exempt, and tax-deferred investors. We find BABs do not exhibit greater liquidity than traditional municipals. BABs are more underpriced initially, particularly for interdealer trades. BABs also show a substitution from underwriter fees toward more underpricing, suggesting that the underpricing is a strategic response to the tax subsidy.

1. Introduction

Build America Bonds (BABs) were created as a part of the American Recovery and Reinvestment Act of February 2009, the fiscal stimulus package passed early in the Obama administration in response to the recession triggered by the financial crisis. The program was offered as an alternative to traditional municipal bonds, which pay interest that is exempt from the federal income tax. While the interest from BABs is taxed as ordinary income, the Treasury refunds 35% of the coupon on the BABs to the issuer.[1] These bonds were popular with issuers. From April 2009 to December 2010, when the program ended, our sample includes 1875 separate underwritings, involving 14,043 separate bond issues of $145 billion in face value. This comprised a quarter of the par value of all municipal issues during the period. Since it ended several proposals have emerged to revive the program. Most recently, in February of 2013, the Obama administration proposed an expanded version of the BAB program, under the name America Fast Forward bonds.

State and local governments can borrow to fund capital investments in infrastructure. Municipalities were seen as a source of "shovel-ready" projects by policy makers, but as constrained by the difficult credit conditions consequent to the financial crisis. The stated purpose of the BAB program was to give municipal issuers access to a "deeper" or "broader" market. The natural clienteles for traditional municipal bonds are taxable individuals along with the mutual funds and trusts that hold the bonds on their behalf, because the bonds are tax-exempt and therefore have low yields. Allowing municipalities to issue taxable bonds, while preserving the benefits of the tax exemption through the coupon rebate, would give municipalities access to a deeper pool of investors, including pension funds and endowments, along with

* Corresponding author: Tel.: +1412 268 2302.
E-mail address: rcgreen@cmu.edu (R.C. Green).

[1] The program authorized two types of bond issues: direct-pay bonds, which work as described in the text, and tax-credit bonds under which the rebate is paid to the bond holder. To our knowledge, based on reports by practitioners and a Bloomberg search, no tax-credit bonds were issued under the program. While it is possible some were, it is clear that they are a tiny fraction of the bonds issued.

0304-3932/$ - see front matter
http://dx.doi.org/10.1016/j.jmoneco.2013.04.010

sovereign funds and foreign investors who do not pay US income taxes. According to the "Fact Sheet" on the Treasury's web site[2]:

> The Build America Bond program has broadened the market for municipal bonds to include investors that do not normally hold tax-exempt debt, such as pension funds and sovereign wealth funds. By attracting new investors to municipal bonds, BABs have helped to relieve the supply pressure in the municipal bond market and lower borrowing costs.

The stated rationale raises several questions. Why should one tax clientele be preferred to another? If the equilibrating flows across taxable and tax-exempt bonds reflect a 35% marginal tax rate, yields on newly issued BABs times (1–0.35) should closely approximate yields on tax-exempt bonds of similar credit and maturity. Issuers should then be indifferent between the two forms of financing, regardless of the "depth" or "breadth" of either set of potential investors. If the relative yields do not reflect a 35% tax rate, why choose that number for the rebate rate?

One economically meaningful interpretation of the stated rationale for the program is that a broader, deeper set of potential investors enhances liquidity—the ability to trade at short notice in large quantities at low cost. Municipal bonds are notoriously illiquid.[3]

One place to look for evidence of improved liquidity is in the underpricing of the new issues. "Underpricing" refers to the extent to which the price eventually paid by final investors exceeds the price at which the security is initially offered to the public. In the current context, a price is set for the bonds shortly before they begin trading. This is known as the "reoffering price" or "reoffering yield." The underwriter buys the bonds from the issuer at this price, lesser a percentage fee, the "underwriter discount." The bonds are then "reoffered" to the public at the stipulated price. Typically, this price is substantially less than the prices at which bonds are sold to customers several days later, as shown in Green et al. (2007b).

Underpricing of new issues is typically described by underwriters as a cost of liquidity or immediacy—the price concession needed to move a large quantity of securities simultaneously. It is also the subject of controversy in the academic finance literature and among practitioners. Underpricing transfers value from issuers to the intermediaries and customers who initially buy the underpriced security. Since underwriters control access to the initial offering, it can also be a means of rewarding favored customers at the expense of the issuers.

A second explanation for the popularity of the bonds is that the structure of the BAB program leads to unintended subsidies. We explore this as a motive for underpricing in the case of the Build America Bonds. Since the tax subsidy is tied to the coupon payment on the bonds, by inflating the coupon level relative to competitive rates, the issuer and underwriter can raise the tax subsidy paid by the Treasury. The higher coupon is a cost to the issuer, and the issuer cannot initially price the bonds at a premium over par value without violating "de minimis" rules, but the present value of the higher coupon can be captured by the underwriters or favored customers who buy the bonds in the secondary market.[4] The underwriter, in turn, can share the benefits of the underpricing with the issuer through lower fees. Thus, viewed as a coalition, the underwriter, the issuer, and investors have a shared interest in underpricing the bonds. We attempt to disentangle the liquidity costs and the strategic tax-induced underpricing by focusing on differences in the pricing of BABs, tax-exempt bonds, and non-BAB taxable municipals issued over the same period.

We examine patterns of trade and costs of trade for BABs, and compare them to traditional municipal bonds issued during the same period. Despite its stated goals the BAB program does not appear to have achieved improved liquidity. The patterns of trade in the secondary market evolve through time in a similar manner for BABs and tax-exempts. The large numbers of retail trades and evidence that the bonds are being flipped by large investors acting as intermediaries are also consistent with the behavior of tax-exempt municipals. Measures of trading activity and trading cost in the secondary market are the same or worse for BABs relative to tax-exempts.

BABs also exhibit more underpricing, particularly after controlling for characteristics such as trade size, par principal of the bond issue, and maturity. Several findings suggest that the greater underpricing may, indeed, be a strategic response to the coupon rebate. We show that the underwriter discount is lower for BABs than for tax-exempts, controlling for characteristics, despite the lower liquidity. The underwriter discount is, in particular, lower for issues that are more underpriced, for BABs but not for tax-exempts. This is consistent with transfer of some of the surplus created by underpricing back to the issuers, who bear the cost of the higher coupons. This negative correlation becomes more pronounced through time, suggesting participants were learning how to respond to the program's structure. Most notably, underpricing of BABs, unlike tax-exempts, is also quite evident in the prices at which dealers trade with each other. These behavior are consistent with a recognition on the part of the issuers and underwriters of a shared tax benefit to the underpricing.

---

[2] See US Treasury web site, http://www.treasury.gov/initiatives/recovery/Pages/babs.aspx.

[3] See for example, Hong and Warga (2004), Harris and Piwowar (2006), and Green et al. (2007a) who all document mean or median implicit spreads on retail-sized trades in excess of 2% using different methods. Green et al. (2007b) document a great deal of price dispersion for newly issued bonds, where there is considerable trade. On some bonds investors pay prices that vary by close to 5% of par value virtually simultaneously. Green et al. (2010) show that prices rise faster than they fall, as they often do in markets for consumer goods. Ang et al. (2011) decompose municipal yield spreads relative to Treasuries into several components, and identify the difference between pre-refunded yields and Treasury yields as due to liquidity.

[4] Under IRS rules, the reoffering price may deviate from par value by the "de minimis" amount, an amount equal to 0.25% of the par value for each complete year until the bond's maturity.

The differences in underpricing are robust to controls for obvious sources of heterogeneity across BABs and tax-exempts. Moreover, we see similar differences between BABs and taxable municipals that were not part of the BAB program. Non-BAB taxable bonds are sold through the same distribution channels and appeal to the same clienteles as BABs, but there is no incentive to underprice them by inflating the coupon to capture a larger tax rebate.

The underpricing of newly issued securities has provided financial economists with a number of interesting and long-standing puzzles. For the most part, these questions revolve around why issuers tolerate underpricing, and how financial intermediaries benefit from it. Initial public offerings of equities are dramatically underpriced, and a long and rich literature speculates on the cross-sectional determinants of the underpricing, theoretical rationales such as adverse selection and signalling, and what indirect benefits issuing firms or investment banks might capture in exchange for surrendering underpriced securities to investors.[5]

Underpricing has also been studied in bond markets. There, because the newly issued securities are often absorbed into underwriter inventories or purchased by hedge funds functioning as "flippers," the underpricing is largely captured by intermediaries. The questions in this setting involve why issuers do not bargain more effectively, how the opacity of the market facilitates underpricing by the intermediaries, and whether the underpricing is compensation for the illiquidity and costs of distributing the bonds to final investors or rents captured by financial intermediaries through market power.[6]

For BABs, the rebate from the Treasury is tied to the coupon rate, reducing the costs of underpricing to the issuer. While the benefit of the underpricing is captured by the underwriters, or any favored intermediaries who act as flippers in the market, the underwriter's spread provides a straightforward means of transferring some of this surplus back to the issuer. Indeed, it appears the Treasury expressed concern about the possibility that the coupons were being set on the bonds at excessively high rates, triggering a vigorous lobbying effort on the part of the issuers, underwriters, and bond counsels in response. We discuss this debate in Section 3.

We are not suggesting that strategic underpricing is the only reason for the BAB program's popularity. It is possible that a tax rate of 35% simply is not the relevant marginal rate for equilibrating flows across the two markets. There is a history of evidence in the finance literature that long-term municipal rates are "too high" to be consistent with high-tax investors being marginal holders in both the taxable and tax-exempt markets. The tax-exempt yield curve is, typically, steeper than the taxable yield curve. Long-term taxable yields, times one minus the tax rate, understate long-term municipal yields of the same maturity.[7] Long-term tax-exempt bonds are an expensive source of financing for municipal issuers, who would prefer to issue taxable bonds with a 35% coupon rebate. The limited amount of formal research on BABs to this point bears this out. Ang et al. (2010) estimate interest savings on newly issued BABs of 54 basis points on average relative to equivalent newly issued tax-exempt municipal bonds, with this benefit increasing in maturity. Thus, our purpose here is not to deny that the BAB program provided less expensive funding to municipalities. It is to evaluate whether the benefit is associated with enhanced liquidity, as suggested by the program's goals, and to explore its unintended consequences.[8]

The paper is organized as follows. Section 2 illustrates how underpricing can collectively benefit the issuer and underwriter, and reviews the public debate about how the Treasury should determine the "interest rate" and the "issue price" on which the rebate is based. Section 3 describes the data and Section 4 evaluates patterns of trade and measures of liquidity for newly issued BAB and tax-exempt bonds. In Section 5 we document the differences in underpricing across BABs, tax-exempt municipals, and non-BAB taxable municipals. We also provide some measures there of the aggregate costs of intermediation for issuers and study the relationship between the underwriter discount and amount of underpricing. Section 6 concludes.

2. Underpricing and the tax subsidy

In this section we illustrate how underpricing creates shared surplus for the issuers and underwriters at the expense of the Treasury. Assume an issuer and underwriter set the terms on a taxable bond, a BAB, of maturity T. The reoffering price R of the bond must be set at a par value of $1 (here we ignore any "de minimis" amount), and the issuer receives this price less the underwriter discount of D. If the bond has a coupon rate of C, a_T is the value of a T-period taxable annuity, and q_T is the value of a pure-discount bond maturing at T, then the value of the bond to investors is

$$V_T = Ca_T + 1q_T. \tag{1}$$

[5] See Jay Ritter's Web Page at http://bear.warrington.ufl.edu/ritter/ for useful summaries of the empirical facts and of the research concerning them.

[6] Green et al. (2007b) show that average prices of municipal bonds rise from the reoffering price, but that this increase is associated with price dispersion and changes in the mix of buyers. Green (2007) argues that capacity constraints in retail distribution networks allow underwriter-dealers to avoid competitive outcomes in negotiating with issuers. Schutz (2012) argues that increased price transparency due to real-time reporting of transaction prices has reduced price dispersion, but has had little effect on average markups to final investors.

[7] Green (1993) documents this behavior, and proposes a model to explain it based on tax-avoidance strategies. Chalmers (1998) shows the behavior is also evident in pre-refunded municipal bonds, which are backed by US Treasury securities.

[8] Advocates for the program also argue that an additional benefit of the program is that the size of the tax benefits do not depend on the marginal rates of the bond holders, and is thus more equitable and efficient. The value of the tax exemption for traditional municipals is higher for high-tax, wealthy individuals. This argument might apply to tax-credit bonds, but these were apparently not used by issuers under the BAB program. It is not clear how the argument would apply to direct-pay bonds. Even with tax-credit bonds, the argument ignores the fact that low-tax inframarginal investors benefit from the tax-exempt status of municipals if higher yields on taxable bonds reflect the differential tax treatment.

This present value can be viewed as what the bond would sell for in frictionless secondary market trading. In our empirical work, we use the inter-dealer price as a proxy for V_T.

The issuer receives a rebate of the coupon equal to τC each period, so the present value of the issuer's liability from the bond is $C(1-\tau)a_T+q_T$.[9] Let C^* be the coupon rate on a bond that would trade at par value in the secondary market—the coupon rate solving $1=C^*a_T+q_T$. Note that $V_T-1=(C-C^*)a_T$.

The underwriter pays the issuer the reoffering price of $1 and receives the discount of D. He then sells the bonds to investors and receives $(1-\phi)1+\phi V_T$, where ϕ is a reduced-form representation of the extent to which the underwriter is able to recapture some of the benefits of the underpricing. This recapture could come in a number of forms. At the extreme, the underwriter might simply sell the bonds from inventory at full value, in which case $\phi=1$. Alternatively, he may sell some of the bonds at the reoffering price, and others later at a higher value. Even if the entire issue is sold at the reoffering price, underpricing might reduce the underwriter's costs of marketing the bonds. Selling underpriced bonds to favored customers might also benefit the underwriter through purchases of other financial services by those customers.

The net liability to the issuer, V_I, and the net benefit to the underwriter, V_U, are

$$V_I=1-D-C(1-\tau)a_T-q_T, \tag{2}$$

$$V_U=(1-\phi)1+\phi(Ca_T+q_T)-1+D. \tag{3}$$

Adding these together, the net surplus associated with issuing the bond is

$$S=\tau Ca_T-(1-\phi)(V_T-1). \tag{4}$$

Alternatively, the issuer and underwriter could issue a bond that sold in the secondary market at par, with coupon C^*, and create a surplus of $S^*=\tau C^*a_T$. The difference between these two expressions, $S-S^*$ gives the benefit (or cost) of underpricing the bond

$$\tau(C-C^*)a_T-(1-\phi)(V_T-1)=[\tau-(1-\phi)](V_T-1). \tag{5}$$

Thus, if none of the benefit of underpricing is captured by the underwriter ($\phi=0$), then the issuer and underwriter have no incentive to underprice the bonds. The tax rebate reduces the cost to the issuer of a higher coupon, but not sufficiently to offset the present value of the greater liability. Similarly, if the tax rate τ is zero, as is the case with traditional, tax-exempt municipals, then there is no incentive to underprice. Even if the underwriters fully capture the value, it is offset one-to-one by the greater liability to the issuer, so that underpricing is purely a transfer from issuer to underwriters or investors. Issuers and underwriters may have a shared interest in underpricing, however, when the tax rebate is positive and some of the present value of the underpriced bonds flows back to them.

The Treasury appears to have expressed some concerns about the possibility that the bonds were being systematically underpriced. In August 2010 an open letter to the Treasury, widely reported in the financial press, was released jointly by the Government Finance Officers Association, the National Association of Bond Lawyers, the Regional Bond Dealers Association, and the Securities Industry and Financial Markets Association. This document complains about actions taken by IRS personnel that “continue to create uncertainty regarding the IRS's interpretation of the rules for establishing ‘issue price’.” The letter urges the Treasury to continue the established practice of treating the reoffering price set through negotiation between issuers and underwriters, and made public in the final pricing wire as the issue price, and claims subsequent secondary market prices cannot be used for this purpose because they are uncertain at the time the bonds are issued:

> The final pricing wire, substantiated by evidence of the offering process, provides the basis for the parties’ reasonable expectations regarding the proper issue price of the bonds, irrespective of the actual sales executed once the bonds become available to investors.

The letter goes on to provide evidence of initial underpricing in other settings and appeals to traditional practices in the tax-exempt setting: “The issue price for tax-exempt bond purposes has been based on the initial offering price to the public for over 25 years. We submit that there is no reason to depart from this approach.” The underpricing in the tax-exempt market is coming at the expense of the issuer, and one might sensibly argue that issuers who fail to effectively bargain with their underwriters have only themselves to blame. Underpricing in the BAB market, however, is coming at the expense of a third party, the tax-paying public.

3. Data

Our data consist of the transaction-by-transaction trade reports for municipal bonds made available through the Municipal Securities Rulemaking Board (MSRB). The MSRB dataset reports every trade carried out through registered broker-dealers in the US, which is virtually the entire public municipal bond market. Trades are reported as sales to

[9] We assume here that the issuer applies the same discount rate to these cash flows as does the taxable bond market. That is, the value of a T-period annuity is what it can be sold for in the taxable market. An alternative derivation, which treats the tax-exempt municipal bond rate as the opportunity cost of capital to be applied to the coupon stream net of the rebate, is available in the on-line appendix to this paper. It relies on the steeper slope of the tax-exempt term structure to arrive at the result.

customers, purchases from customers, and inter-dealer trades. For each trade there is a price, the par value of the bonds exchanged, and the time at which the trade took place. When new bonds are issued, the MSRB data do not include the transfer of the bonds from the issuers to the underwriters. Thus, the first trades we see are sales from the underwriters to customers, or possibly to other dealers.

Along with the transactions data, we have information about specific bonds and issuers from two sources: SDC Platinum, and a database collected and made available to us by Primuni.com. The latter was hand collected from the official statements and information provided on the MSRB's Electronic Municipal Market Access (EMMA) website. We use both of these databases to check inconsistencies and resolve missing data where possible. The data include issuer characteristics (such as name, state, type), reoffering prices or yields for each bond issue, and issue characteristics (such as maturity, coupon, call schedule, taxable status, stated use of funds, and sinking fund provisions). These data also provide information on the underwriting syndicate, including underwriters' names and underwriting fees. The filters we apply to clean the data are similar to those in Green et al. (2007b). The on-line appendix provides a detailed description of how records with missing or clearly incorrect data were treated, and an accounting of steps that lead to our final sample of 161,468 bonds, or CUSIPs, and 3,216,300 trades.[10] The first issuance in our sample was on May 12, 2009, and the last was on December 29, 2010.

Municipal bonds are typically issued "in series," and this is often cited as a source of their low liquidity.[11] Multiple bonds with different maturities are underwritten simultaneously in one "deal." Each maturity has a separate CUSIP number and trades as a separate security in the secondary market. The separate maturities in a deal are commonly referred to as "CUSIPs," bonds with the same terms and initial price.

Panel A of Table 1 shows that there were fewer BAB deals over the sample period than traditional tax-exempts, but BAB deals were substantially larger. There were also fewer individual CUSIPs per deal. Liquidity requires coincident needs between large numbers of potential buyers and sellers, and this is, of course, facilitated by a greater supply of identical bonds. Thus, we would expect that the larger deals, with fewer CUSIPs per deal, enhance liquidity, independently of the investor clientele.

Occasionally, multiple deals are combined in a single underwriting, managed by the same underwriter for the same issuer. For example, we have 984 instances where a municipality issues both BABs and tax-exempts at the same time through the same underwriting syndicate. We exploit these situations in some of our analysis.

4. Market liquidity measures

Panel B of Table 1 provides statistics on the overall patterns of trade over the first 60 days of trading. For both types of bonds, volume is dominated by sales to customers. The median sale by dealers to customers is smaller for BABs than for tax-exempts. This is striking considering that one of the stated purposes of the program was to give municipal issuers access to institutional investors, such as endowments and pension funds, that trade in larger quantities.

When we consider the cross section of CUSIPs rather than the cross section of trades, we see that trading activity is highly concentrated in a small number of bonds. The dramatic differences between medians and means of trades per CUSIP show trading activity for both BABs and tax exempts is highly skewed, particularly for the BABs. The next line in the table shows that the median over all CUSIPs of the median trade size within each CUSIP is somewhat larger for the BABs than for tax-exempts. This suggests that many of the CUSIPs are indeed being placed with institutional investors, as intended. The small number of trades for the median CUSIP shows that, for both BABs and tax-exempts, relatively little trading is involved in distributing many of the bonds to their final holders. The typical bond is quickly sold off in large blocks to institutions. Most of the trading activity is associated with a subset of bonds that are widely distributed to smaller investors. These tend to be associated with larger, more visible issuers in bigger deals.

The financial press and the regulatory authorities have frequently expressed concern over "flipping" of new municipal bonds by large intermediaries such as hedge funds. Customers who flip municipal bonds are effectively performing the underwriters' function of distributing the bonds to final customers. They buy large blocks of bonds from a dealer, and then sell them to other dealers with retail distribution capability, who in turn sell them to retail investors that buy and hold the bonds. Since the stated purpose of the BAB program was to provide issuers with access to a more liquid market, we might expect less need for flipping, since they can be purchased and held directly by large institutional investors. On the other hand, since the final customers for BABs are not the traditional clientele for municipal issuers and underwriters, we might alternatively expect more intermediation by third parties as the bonds make their way through new distribution channels to the final investors.

A simple means of detecting flipping of newly issued bonds is to ask if the par value of total sales to customers over the first 60 days of trading exceeds the par value of the issue. If that is the case for a given CUSIP, then evidently some customers are buying the bonds, selling them back to dealers, who are in turn selling them to other customers. A less conservative measure of the bonds being recycled through dealers is the ratio of sales to customers over total "underwriter sales," defined

[10] This includes 7075 taxable bonds with no coupon subsidy that we only use in certain portions of the analysis. We exclude 5073 taxable bonds with possible coupon subsidies (45%, 35% or 0% coupon subsidy) we were unable to verify or were different than 35%, which we do not use in any of our tests.

[11] See for example, Ang and Green (2011).

Table 1
Descriptive statistics on deal and trade characteristics. Multiple CUSIPs, or bond issues, are underwritten simultaneously in a deal. Panel A reports descriptive statistics for deals in our sample for Build America Bonds (BABs) and traditional tax-exempt municipals. Panel B reports descriptive information for trades in those bonds over the first 60 days. Panel C reports ratios of sales-to-customers to par value of the issue or to total sales of the bond by underwriters. Underwriter sales are defined as the par value of the issue less bonds that remain in dealer inventories after the first 60 days of trade. We define bonds that have been "flipped" are bonds where the ratio of total sales to underwriter sales exceeds one. All differences in means between BABs and tax-exempts (indicated by ∗) have levels of significance beyond 0.01%.

Statistic	BAB	Muni
Panel A: deals		
Number of deals	1875	13,554
Number of CUSIPs	14,043	140,350
Mean number of CUSIPs per deal*	8.1	11.4
Mean par value per CUSIP ($ M)*	10.3	2.8
Median par value per CUSIP ($ M)	1.14	0.52
Mean deal value ($ M)*	80.9	27.7
Median deal value ($ M)	20.8	5.8
Panel B: trades ≤60 days after issuance		
Number of trades (thousands)	620	2422
Number of sales to customers (thousands)	478	1629
Median par value of sales ($ K)	15	30
Median number of sales per CUSIP	3	3
Mean number of sales per CUSIP*	34	12
Median across CUSIPs of median par value of sales	160	104
Panel C: flipping		
Mean total sales/par value of new issues*	1.05	1.03
Mean total sales/underwriter sales*	1.08	1.04
CUSIPs with flipping by customers (%)*	26.4	15.6
Panel D: trades >60 days after issuance		
Percent of CUSIPs with sales	29%	25%
Number of trades >60 days/trades <60 days	24%	28%
Number of sales to customers >60 days/sales <60 days	17%	20%
Median par value of sales ($ K)	15	25
Median number of sales per CUSIP	4	3
Mean number of sales per CUSIP*	19	9
Median across CUSIPs of median par value of sales	25	33

as the difference between the par value of an issue and the par value of bonds still in dealer inventory at the end of 60 days. Suppose, for example, a dealer initially places 85% of the bonds in a specific new issue with one hedge fund, and the dealer is unable to sell the remaining bonds, which remain in inventory. If the hedge fund sells its bonds to regional broker-dealers, who in turn sell them to retail investors, then sales will be two-times underwriter sales, which means all the bonds the dealer has sold have been flipped. None of them end up with the investors who first purchased them.[12]

Panel C of Table 1 shows a similar picture emerges for either measure of flipping. For each CUSIP in the sample, we compute the ratios of total sales to par value and total sales to underwriter sales, and report the means across CUSIPs. For both tax-exempts and BABs the averages exceed one, and they are slightly higher for BABs, statistically significantly so at less than 1%. We define bonds that have been flipped as those where total sales over underwriter sales exceed one, and the table shows that 26% of the BABs have this characteristic, while only 16% of tax-exempts issued during the same period of time show evidence of flipping. This difference is economically and statistically significant. The greater prevalence of flipping for BABs may be due to the larger deals and fewer CUSIPs per deal. The size of the issues may require dealers to enlist and compensate non-broker-dealers in the distribution process. If higher liquidity is the reason the BABs are turning over more in the first 60 days of trade, then we would expect less underpricing for those bonds. On the other hand, if the reason the bonds are turning over is because of limited access to the final customers, which one would associate with lower liquidity, then we would expect the prices eventually paid by customers to be at higher percentage markups over the reoffering price, or greater underpricing, for BABs. We consider underpricing in the next section.

Trade in the days immediately following the issuance of the bonds combines trade between secondary market participants and the movement of the bonds from the inventories of the underwriters to investors. Investors, when they purchase the bonds, will ultimately pay less for bonds they expect will be costly or difficult to sell in the secondary market, should they find the need to do so. Activity in the bonds after 60 days of trade is informative about the liquidity investors

[12] We exclude from the sample in these calculations CUSIPs with final inventory greater than the CUSIP's par principal. Missing sales to customers will increase the ratio of total sales to underwriter sales. We do not include CUSIPs with ratios that are obviously too large (i.e., greater than 10). When underwriter sales are greater than the CUSIP principal (i.e., negative final inventory), we set them to be equal to the CUSIP's par principal. This might underestimate the ratio of total sales to underwriters sells. Obviously, these procedures would only bias our comparison of the BABs and tax-exempts if omissions or other data errors are more prevalent in one case than another, which seems unlikely given that the same issuers, regulators, and underwriters are involved.

should be most concerned about. Greater secondary market liquidity for BABs, because they are aimed at a different investor clientele, would provide a rationalization for the program consistent with its stated objectives.

Panel D of Table 1 shows mixed evidence on the differences in trading activity between BABs and tax exempts after the first 60 days of trade. The BABs show a slightly larger fraction of CUSIPs with sales to customers and more sales per CUSIP, but the median trade sizes are lower and there is a larger decline in trade relative to the first 60 days. The difference is evident from comparing the ratios of the number of trades and sales to customers after 60 days to the equivalent measures of volume over the first 60 days of trading (lines two and three in Panel D).

In Table 2 a clearer distinction between BABs and tax exempts emerges when controlling for deal and CUSIP characteristics. Each column presents the results of regressing a different measure of liquidity on a BAB dummy and a large number of controls. Panel A covers the first 60 days of trade and Panel B the trades after 60 days. Panel B includes probit and logit regressions for the probability that any trade at all occurs (omitted in Panel A because all bonds in our sample show some trade initially). The coefficients on the BAB dummy represent the increase in liquidity for the BABs relative to the tax-exempts issued over the same period. Over the first 60 days, the coefficients on the BAB indicator are insignificant for two of the four measure, marginally significantly negative for one, and significantly positive for trade size as a fraction of size of the issue.

After 60 days, the BABs appear to trade significantly less than the tax-exempts controlling for characteristics. Panel B shows the coefficients on the BAB indicators are significantly negative for all but the trade size measure, for which they are insignificant. Bonds issued earlier in our sample are mechanically more likely to show trading activity after 60 days than bonds issued later on. If the timing of the issues differs across the two types of bonds, then this could affect our results. We show in Appendix C of the on-line appendix to the paper that our results are robust to controlling for this possibility, by reporting details of logit and probit regressions for the probability of trade in various amounts that include indicators for quarters and interaction terms between these time and BAB dummies.

Table 2
Determinants of BAB liquidity. The table reports the determinants of BAB liquidity. Each bond issue is an observation. Turnover is total volume over issue size, log transformed. No. trades is total number of trades, log transformed. Trade size is the average par transaction size. All specifications include state, month of issuance, issuer type, and use of proceeds fixed effects. Standard errors are clustered by month of issuance. Levels of significance are denoted by * (5%), ** (1%), and *** (0.1%).

Determinant	Turnover	No. trades	Trade size	Trade size/Issue size	Pr(trade) probit	Pr(trade) logit
Panel A: trades ≤60 days after issuance						
BAB	−0.46	−6.03	−0.13*	0.04***	–	–
Maturity	1.10***	6.13***	0.01***	−0.01***	–	–
Rating	0.13	3.03***	−0.04***	−0.00*	–	–
Unrated	−3.09***	6.57***	0.15*	−0.00	–	–
Insured	9.47***	24.65***	−0.17***	−0.03**	–	–
Callable	1.28*	−8.12***	−0.42***	0.03***	–	–
Sinkable	−0.03	−1.52	−0.10**	0.02**	–	–
Fixed coupon	−2.83***	−4.31*	0.17***	−0.01	–	–
Extraord. redemption	0.56	8.76**	−0.08*	−0.02*	–	–
ln(deal size)	0.36	35.94***	0.59***	−0.11***	–	–
No. CUSIPs in deal	0.18***	−1.57***	−0.04***	0.00***	–	–
Competitive offering	4.41***	−1.57	0.36***	0.02**	–	–
Refunding bond	1.08*	2.61*	−0.14***	−0.00	–	–
Advanced refunding	−2.16*	−7.36*	−0.09*	−0.00	–	–
Constant	78.88***	75.01***	0.18	0.77***	–	–
R^2	0.119	0.374	0.116	0.154	–	–
N	153,819	153,819	153,819	153,819	–	–
Panel B: trades >60 days after issuance						
BAB	−1.66**	−11.05***	−0.03	0.01	−0.14***	−0.24***
Maturity	0.29***	2.91***	0.00	−0.00***	0.03***	0.06***
Rating	−0.30**	0.40	−0.01**	−0.00	−0.01	−0.02
Unrated	−1.94***	0.59	0.09*	0.00	−0.13***	−0.23***
Insured	−1.23**	−1.71	−0.04**	−0.00	−0.06**	−0.10**
Callable	−1.52***	−8.56***	−0.13***	0.01	−0.18***	−0.32***
Sinkable	−0.73***	−2.86**	−0.03*	0.02***	−0.09***	−0.16***
Fixed coupon	0.06	−1.43	0.02	−0.00	0.01	0.02
Extraord. redemption	−0.56	−0.89	−0.01	−0.01*	−0.01	−0.01
ln(deal size)	3.33***	27.57***	0.14***	−0.05***	0.46***	0.78***
No. CUSIPs in deal	−0.08**	−1.23***	−0.01***	0.00***	−0.02***	−0.03***
Competitive offering	0.54	−2.24	0.09**	0.01*	−0.04*	−0.07*
Refunding bond	0.66**	5.47***	−0.04	−0.00	0.07***	0.12***
Advanced refunding	0.88	−0.16	−0.00	−0.00	0.10***	0.18***
Constant	19.07***	101.50***	0.08	0.25***	−1.06***	−1.83***
R^2	0.101	0.329	0.042	0.098	0.252	0.251
N	153,819	153,819	39,591	39,591	147,737	147,737

Table 3
Determinants of BAB trading cost. The table reports the determinants of BAB trading cost. Each trade is an observation. We estimate the following specifications for all trades i

(1) $\Delta Price_i = \alpha + \beta \Delta \text{TradeSign}_i * \text{BAB} + \gamma \Delta \text{TradeSign}_i + \delta \Delta Par_i * \text{TradeSign}_i * \text{BAB} + \xi X_i + \epsilon_i$

(2) $Price_i / Benchmark_i = \alpha + \beta \text{TradeSign}_i * \text{BAB} + \gamma \text{TradeSign}_i + \delta Par_i * \text{TradeSign}_i * \text{BAB} + \xi X_i + \epsilon_i$

TradeSign $_i$ takes the value plus (minus) one for a customer buy (sell), and zero otherwise. $Benchmark_i$ is the last interdealer price or the reoffer price if no interdealer has occurred. The coefficient γ measures the average trading cost for munis, and β measures the difference in trading cost between BABs and munis. All specifications include state, month of issuance, issuer type, and use of proceeds fixed effects. Standard errors are clustered by issue and time. Levels of significance are denoted by * (5%), ** (1%), and *** (0.1%).

Determinant	Trade date ≤60 days		Trade date >60 days	
	(1)	(2)	(1)	(2)
BAB *TradeSign	0.05**	0.19***	0.28***	0.22***
TradeSign	0.96***	0.75***	0.88***	0.98***
BAB *TradeSign *Par	0.00	−0.01	−0.01	−0.01
TradeSign *Par	−0.02***	−0.02***	−0.07***	−0.10***
Par	0.01*	−0.01**	0.01	−0.04***
BAB	0.01***	0.01	0.05***	0.05
Maturity	0.00	0.04***	−0.00	0.03***
Rating	0.00	0.01	0.00**	0.03***
Unrated	0.00	0.05*	0.04***	0.23***
Insured	0.01***	0.09***	0.03***	0.02
Callable	0.01***	0.04	0.01*	0.02
Sinkable	0.00*	0.08***	0.01*	0.04
Fixed coupon	−0.00	−0.01	−0.01**	−0.07*
Extraord. redemption	0.00*	−0.05	0.00	0.08*
ln(Deal Size)	−0.00***	0.01	−0.02***	−0.01
No. CUSIPs in deal	0.00*	0.00	0.00**	0.00
Competitive offering	−0.02***	−0.09***	0.00	−0.04
Refunding bond	−0.01***	0.01	−0.00	0.01
Advanced refunding	−0.01*	−0.03	−0.01	−0.03
Constant	−0.54***	2.79***	0.23	0.32
R^2	0.399	0.359	0.375	0.215
N	2,860,293	2,166,486	798,992	546,423

The amount of trading activity should be associated with the ease with which market participants can identify counterparties. What investors are ultimately most concerned about, however, is the cost of trade. Table 3 shows that, controlling for characteristics of deals and CUSIPs, trading costs are higher for BABs than for tax-exempt bonds issued over the same period. We use two measures of trading costs that have been employed in the literature on OTC markets. The first follows Schultz (2001). We regress the price changes from one trade to the next on the change in the sign of the trade. For example, if the previous trade was a buy from a customer (−1) and the current trade is a sale to a customer (+1), the independent variable would be +2 (= (+1)−(−1)). We would expect the price to increase in this situation. In the reverse situation, the change in the trade sign is −2, and we would expect the price to decrease. The coefficient on this variable, therefore, measures the average half-spread between the prices at which customers buy or sell and the mid-point. We interact the change in the trade sign variable with a BAB indicator to measure the increase or decrease in trading costs for BABs. The second specification follows Hendershott and Madhavan (2012). It regresses the ratio of the transactions price over a benchmark price, which proxies for the midpoint or common value of the bond, against the sign of the trade. This ratio should be higher for customer purchases than sales. The benchmark we employ is the most recent interdealer price, or the reoffering price if no interdealer trades have occurred.

The first line of Table 3 shows that average trading costs are significantly higher for BABs, both before and after 60 days of trading. The three-way interaction that combines BAB and trade sign with the par value of the trade has an insignificant coefficient (third line of the table), which makes clear that the higher costs for the BABs are not attributable simply to differences in trade size. The estimates of the increased trading costs for the BABs are robust to excluding the control variables.[13]

To summarize our results on liquidity, then, the amount of secondary trading activity is not higher for BABs (Table 1, Panel B) and controlling for characteristics of the bonds it is actually lower (Table 2). Trading costs are, on average, significantly higher for BABs in both issuance period and after 60 days (Table 3). If BABs offer less expensive financing for municipalities, it does not appear to be due to higher secondary market liquidity.

[13] The coefficient estimates for the interaction term with the controls excluded are 0.04, 0.24, 0.27, and 0.20. The first is significant at the 10% level and the others at the 1% level. These are very similar to the results in the first line of Table 3.

5. Underpricing of Build America Bonds

We now turn to the differences between the behavior of the BAB prices and those of the tax-exempts in relation to the reoffering price, which is set by the underwriter. Our measure of underpricing is the difference between the average price paid by customers and the reoffering price, as a percentage of the reoffering price. We will refer to this quantity as the "markup" or "percentage markup" over the reoffering price. Note this may or may not correspond to the markup earned by the dealer intermediating that specific trade, since the dealer may not have actually bought the bonds at the reoffering price.

5.1. Amount of underpricing

Fig. 1 shows average markups over the reoffering price for the cross section of new issues during the sample period, and various subsamples. The top two figures plot the average percentage markup by day from the start of trading and by type of trade. The BAB bonds are on the left-hand side, and the tax-exempt bonds are on the right. The horizontal line in the figures represents a zero markup—that is, sales to customers at the reoffering price. For each day after the start of trade, we compute the average markup over the reoffering price for each CUSIP for which trades occur on that day. We then average across all of these CUSIPs. This gives bonds for which relatively few trades occur equal weight with those for which there was heavy volume, and is thus more representative of the typical bond.[14]

The figure shows that for traditional municipals the average price at which sales to customers occur rises after the first day, while the interdealer trades and purchases from customers are much more stable through time and close to the reoffering price. In contrast, all the prices continue to rise for the BABs after the first day, and the initial underpricing is much larger.

Green et al. (2007b) show that the gradual rise in the prices at which bonds are sold to customers is associated with increasingly smaller trades, more price dispersion, and fewer trades at the reoffering price. The frequency of small trade sizes and the similarity of patters of trade evident in Table 1 during the first 60 days of trade suggest that despite the different target investor clientele, analogous forces are at work for the BABs in the increasing spread between prices for the sales to customers and interdealer trades for the BABs. This is consistent with anecdotal reports that many of the BABs were sold to retail customers for their IRA accounts.

While both types of bonds show a rise through time in the retail price relative to the interdealer price, the BABs also have larger total markups over the reoffering price for the interdealer trades and purchases from customers. These markups rise steadily through time.

A difference between the prices at which bonds are finally sold to customers and the cost of the bonds to dealers could be due to several factors. The dealers face costs reaching final customers through their distribution network for which they must be compensated (Schutz, 2012). They may, alternatively, have market or bargaining power when negotiating with issuers initially and investors subsequently. Either the costs of intermediation or the rewards to market power could vary across BABs and tax-exempt bonds. The decision to issue one type of bond versus another is endogenous. It is made in light of anticipated liquidity and credit risk, and is thus likely to be correlated with both observed and unobserved heterogeneity. Thus, we should not find it surprising to observe differences in the relationship between final prices and dealer costs for the two types of bonds.

It seems, however, that the most dramatic differences in the evolution of prices are in the degree to which the BABs are initially underpriced relative to the terms at which dealers trade with each other. A strategic response by underwriters and issuers to the tax incentive to inflate the coupon level is one possible explanation for this behavior.

Given the large numbers of CUSIPs in our sample, it is not surprising that the differences evident in these figures are statistically significant. Panels A and B of Table 4 provide statistics, for BABs and tax-exempt municipals, respectively, for the par-weighted average price at which bonds are sold to customers (P) over the first 60 days, the average interdealer price (V), and the reoffering price (R). For each CUSIP, we first compute the par-value weighted average price. In contrast to the plots in Fig. 1, here we pool all observations across days. The table reports the mean and standard deviation of this number across CUSIPs, and similarly for V and R. The average differences between these prices are all statistically significant at high levels of confidence. The bottom panel of the table reports the same information for taxable municipals that are not part of the BAB program, discussed below.

5.2. Trade size, maturity, issuer effects and non-BAB taxable municipals

The evidence that the BABs are as underpriced, or more underpriced, than tax-exempt municipals is subject to the caveat that the issuers and underwriters are choosing to issue BABs in light of anticipated liquidity, credit risk, and other sources of heterogeneity through time and across issuers. The remaining rows in Fig. 1 show that the difference between the BABs and tax-exempt municipals survive controls for the most obvious sources of heterogeneity.

As we discussed in the introduction, the term structure is steeper for tax-exempt bonds than it is for taxables. BABs, therefore, are more likely to be issued for longer maturities. Indeed, in some instances during the program tax-exempt

[14] The same plots of average markups with every trade weighted equally has a very similar appearance, as is evident from the bottom row of Fig. 1, which weights all trades equally within trade size category.

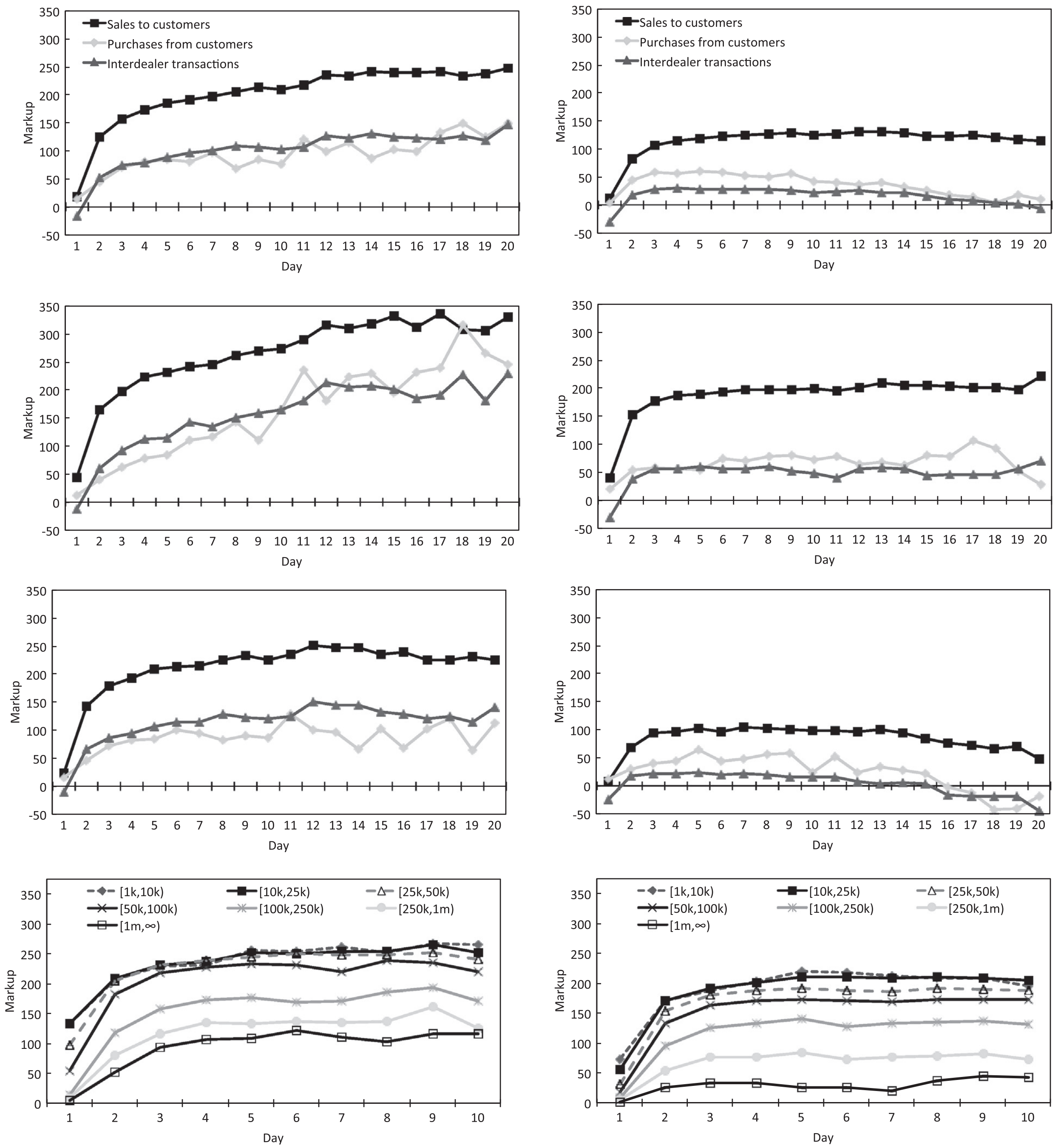


Fig. 1. Evolution of markup over reoffering price by day from initial trading. In each row the plot on the left describes BABs and the plot on the right tax-exempts. The first row plots cross sectional averages of mean markups in basis points for all bonds in the sample. The second row includes only bonds with fixed maturity of 20 years. The third row includes only deals where BABs and tax-exempts were issued simultaneously. The final row provides average markups in basis points for all transactions stratified by trade size.

bonds and BABs were issued simultaneously in series, with traditional municipals at the shorter maturities and BABs and the longer maturities. If underpricing is associated with illiquidity, then longer maturities are likely to be more underpriced.

The second row of plots in Fig. 1 shows the evolution of markups over the reoffering price for bonds with a fixed maturity of 20 years.[15] As with the first row, these plots show averages across CUSIPs of daily average markups over the reoffering price on trades for each CUSIP. The panel on the left is for BABs and the one on the right is for tax-exempts. Note that the

[15] Plots for fixed maturities of 10 and 30 years look very similar.

Table 4
Measures of the costs of intermediation. The table reports descriptive statistics on underpricing and the costs of financial intermediation for three types of municipal bonds. Each CUSIP is an observation. P is the value-weighted average price at which bonds are sold to customers for the CUSIP. V is the value-weighted average interdealer price, set to the reoffering price if no interdealer trades occur. R is the reoffering price for the CUSIP. Aggregate cost is the average across all CUSIPs of principle times the underpricing, times the number of CUSIPs. Since the underwriter discount is often missing, we approximate the aggregate fees by multiplying the average cost per CUSIP by the total number of CUSIPs. Levels of significance are denoted by * (5%), ** (1%), and *** (0.1%).

Measure	Mean	SD	Aggregate ($ B, % of principal)
Panel A: BABs (N=14,043)			
P ($)	100.95	1.25	–
V ($)	100.17	0.92	–
R ($)	100.12	0.57	–
$P–R$ ($)	0.83***	1.20	2.79 (1.92%)
$V–R$ ($)	0.05***	0.80	0.58 (0.40%)
$P–V$ ($)	0.78***	1.00	2.21 (1.53%)
Underwriter spread ($ K)	88.56	445.81	1.24 (0.86%)
Panel B: tax-exempt muni (N=140,350)			
P ($)	103.12	4.70	–
V ($)	102.43	4.75	–
R ($)	102.61	4.76	–
$P–R$ ($)	0.51***	0.84	3.50 (0.89%)
$V–R$ ($)	−0.18***	0.82	−0.41 (−0.10%)
$P–V$ ($)	0.69***	1.01	3.90 (0.99%)
Underwriter spread ($ K)	23.70	125.70	3.33 (0.84%)
Panel C: taxable muni (N=7075)			
P ($)	100.85	1.58	–
V ($)	100.25	1.44	–
R ($)	100.23	1.39	–
$P–R$ ($)	0.62***	1.01	0.41 (1.22%)
$V–R$ ($)	0.02**	0.66	0.07 (0.22%)
$P–V$ ($)	0.60***	0.84	0.33 (1.01%)
Underwriter spread ($ K)	38.19	177.48	0.27 (0.82%)

basic behavior evident for the overall sample are also present here. Fixing the maturity, BABs appear to be as underpriced or more underpriced than the tax-exempt bonds. In particular, the BABs show more evidence of larger markups over the reoffering price for interdealer trades and purchases from customers.

Unobserved heterogeneity across issuers potentially contaminates some of the comparisons described above. While our regression analysis below controls for many factors, there are thousands of issuers, and it is difficult to obtain data on their characteristics that could fully account for differences in credit risk, familiarity to investors, and transparency in reporting that might bear upon a given bond's liquidity. The data provides something of a natural check on the robustness of the results on this dimension because many BABs were issued simultaneously with tax-exempt bonds in a single underwriting.

In the third row of plots in Fig. 1, we reproduce the top row of the figure, but we limit the sample to BABs and tax-exempts issued at the same time by the same municipality. We have 984 cases where tax-exempts and BABs from the same issuer have exactly the same sale date. Again, the central behavior evident for the overall sample are present here. The BABs show more price appreciation through time, especially when considering the interdealer prices.

BABs are issued in larger deals with fewer CUSIPs, which should enhance liquidity. They were also intended to appeal to institutional investors, such as pension and sovereign funds. They should, therefore, trade in larger quantities. The final row of Fig. 1 stratifies the markups over the reoffering price on sales to customers by the par value of the trade. For every trade-size category, the initial underpricing is higher for the BABs, and the increases in price appear to continue for a longer time. The contrast between the BABs and the tax-exempts is most dramatic for the largest trades. This reinforces the evidence that the BABs are underpriced relative to interdealer trades and purchases from customers. The underpricing of the BABs is most apparent in the institutionally sized trades. This points to strategic reasons for underpricing, rather than underpricing as a concession to liquidity or as compensation for the costs of reaching small investors.

Another difference between traditional municipals and BABs is the sales and trading desks within the underwriting firms that intermediate their distribution. The BABs, because they are being sold to a taxable clientele, are typically handled by the corporate trading desk, when the underwriter or dealer is large enough to separate these functions. Since compensation and institutional practices may vary across desks, this might explain the differences in the underpricing evident in Fig. 1.

Municipal entities, however, have frequently issued bonds that do not qualify for tax-exempt status. For example, a number of municipalities have issued debt to fund their pension plans. Since the pension invests in taxable securities, the Treasury does not allow debt issued for this purpose to qualify as tax exempt. For these bonds, however, there is no incentive to underprice due to a tax rebate on the coupon.[16] Panel C of Table 4 shows that these bonds behave more like tax-exempt municipals than BABs. The par-weighted average interdealer price is within two cents, per $100, of the reoffering price. The average difference between the price at which the bonds are sold to customers, and the interdealer price is actually lower than for either BABs or tax-exempts.

5.3. Aggregate costs of intermediation

Any underpricing of the bonds represents a cost of financial intermediation to the issuers. It may be "money left on the table," due to inattention by the issuer, or it may represent market power for the underwriters. It may also be an alternative form of compensation for the underwriter and other intermediaries for the costs of identifying and distributing bonds to the final investors with the highest valuations. The BABs, however, are also underpriced relative to the interdealer market. Since we do not see evidence of underpricing in the interdealer market for tax-exempts and non-BAB taxable municipals, it seems less plausible that this form of underpricing for BABs would be simply a cost of reaching retail investors.

Table 4 provides some summary measures of these costs for the issuers during the 20 months of the BAB program. The final column reports, for each measure of underpricing, the aggregate amount summing across all issues in billions of dollars. We also provide an estimate of the total fees paid through the underwriter discount. In parentheses we report each of these as a percentage of the principal value of the debt. The underpricing of the BABs amounted to $2.79 billion, of which $0.58 billion was due to the difference between the average interdealer price and the price at which the bonds were reoffered to the public. In addition, issuers paid about $1.24 billion in underwriting fees. On the tax-exempt bonds issued during the same period, $395 billion, the underpricing cost issuers $3.50 billion, as a percentage of face value half as much as with BABs. Underwriting fees, at $3.33 billion were slightly lower for tax-exempt bonds as a percentage of principal. The analysis in Section 2 suggests that the cost of the coupon inflation to the Treasury can be viewed as approximately the tax rate times the value of the underpricing as reflected in the interdealer market, or 35% of $580 million. While not a large amount by the norms of the federal budget, this would, of course, become much larger if the BAB program were permanently reinstated and came to be a preferred form of debt financing by municipal issuers, and if no steps were taken to control the underpricing.

5.4. Determinants of markups over the reoffering price

The characteristics of a bond deal, its pricing, and whether to use tax-exempts or BABs are determined simultaneously and endogenously. An attempt to deal with the resulting possible selection bias is made in Tables 5–7. The first of these provides estimates of probit and logit specifications that use characteristics of the bond to predict whether the municipality chooses to issue BABs or tax-exempt bonds. The results are very similar across specifications. BABs are likely to be longer maturity, more likely to be rated, and are issued in larger deals. We then examine the determinants of the percentage markup on interdealer trades (Table 6) and sales to customers conditional on the sale not occurring at the reoffering price (Table 7). In each case, the deal is the unit of observation, and the characteristics of the component CUSIPs within a deal are par-value weighted. The first three columns report results of OLS regressions, controlling for progressively larger sets of characteristics. The probit model from the second column of Table 5 is then used to control for selectivity (the "Heckman correction") in the final column.

The coefficient on the BAB dummy variable in these regressions measures the underpricing controlling for characteristics and selectivity. The first column shows that, unconditionally, BABs are more underpriced than tax-exempts by roughly half a percent in the interdealer market, and by three-quarters of a percent when sold to investors. These magnitudes fall to around 30 and 25 basis points, respectively, when controlling for bond and deal characteristics, and these measures of the underpricing are affected very little by the control for selectivity.

These differences are not surprising given the different reasons for underpricing. The price dealers charge each other provides a natural measure of the intrinsic value of the bond. If the bonds are being underpriced strategically, due to tax incentives, this should be evident in the prices dealers pay each other, as we see in the BABs. If underpricing is compensation to dealers and other intermediaries for the costs of distributing the bonds to final investors, or a concession to liquidity, then we should see underpricing evident in prices those investors pay when they buy the bonds, but not in the prices dealers pay each other. This is what we see in traditional tax-exempts. Hence the difference between the markups over the reoffering price to investors for BABs and tax-exempts is actually less than the difference in markups on interdealer trades.

5.5. The underwriter discount

Underwriters are compensated through fees, as well as any profits they can earn on newly issued bonds they absorb into inventories. The fees, known as the underwriter "discount" or "gross spread," are specified in the official statement for the

[16] Other non-BAB taxable bonds include bond issued under smaller programs that included a direct-pay feature, such as Recovery Zone Bonds, which had a 40% rebate rate. We exclude these from our sample of taxable bonds.

Table 5
Determinants of BAB issuance. The table reports the determinants of BAB issuance. Each bond deal is an observation, and characteristics of CUSIPs within a deal are value-weighted. Specifications (A) and (B) are estimated using a probit model. Specifications (C) and (D) are estimated using a logit model. All specifications include state fixed effects and month of issuance fixed effects. Standard errors are clustered by month of issuance. Levels of significance are denoted by * (5%), ** (1%), and *** (0.1%).

Determinant	(A)	(B)	(C)	(D)
Maturity	0.04***	0.06***	0.07***	0.12***
Rating	−0.13***	−0.10***	−0.25***	−0.19***
Unrated	−0.96***	−0.73***	−1.84***	−1.37***
Insured	−0.21**	−0.22*	−0.41**	−0.44**
Callable	0.54***	0.56***	1.06***	1.09***
Sinkable	0.35***	0.31***	0.66***	0.58***
Fixed coupon	0.04	−0.02	0.03	−0.06
Extraordinary redemption	0.88***	1.13***	1.57***	2.14***
ln(deal size)	0.18***	0.31***	0.32***	0.57***
Number of CUSIPs in deal	−0.06***	−0.07***	−0.11***	−0.13***
Competitive offering	0.24*	0.07	0.46*	0.11
Refunding bond	−3.05***	−3.13***	−6.79***	−7.11***
Advanced refunding	0.68	0.44	2.08	1.91
Constant	−0.11	−2.49***	0.08	−4.59***
State F.E.	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes
Issuer type F.E.		Yes		Yes
Use of proceeds F.E.		Yes		Yes
N	15,252	15,179	15,252	15,179

Table 6
Determinants of interdealer markups. The table reports the determinants of the markups on interdealer trades. Markups on interdealer trades are constructed as the mean markup over the reoffering price on all interdealer transactions weighted by the trade par. The dependent variable is scaled by the reoffering price and expressed in basis points. Markups and characteristics are value weighted by bond deal. Specifications (A)–(C) are estimated using OLS. Specification (D) is estimated using a treatment effect model. All specifications include state fixed effects and month of issuance fixed effects. Standard errors are clustered by month of issuance. Levels of significance are denoted by * (5%), ** (1%), and *** (0.1%).

Determinant	(A)	(B)	(C)	(D)
BAB	50.60***	35.82***	31.81***	36.21***
Maturity		0.80	0.94*	0.89***
Rating		−0.04	0.07	0.14
Unrated		−2.86	−2.03	−1.66
Insured		18.23v	18.47***	18.64***
Callable		−1.08	−1.10	−1.31
Sinkable		2.49	3.46	3.26
Fixed coupon		−7.72***	−7.84**	−7.89***
Extraordinary redemption		−1.37	1.41	0.30
ln(deal size)		1.13	2.45	2.23**
Number of CUSIPs in deal		−0.96***	−0.96***	−0.91***
Competitive offering		−52.33***	−54.06***	−53.96***
Refunding bond		4.61	4.04	4.52*
Advanced refunding		−9.35	−10.43*	−10.21**
Constant	−67.86**	−60.92**	−76.27**	−76.90***
State F.E.	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes
Issuer type F.E.			Yes	Yes
Use of proceeds F.E.			Yes	Yes
R^2	0.156	0.234	0.240	–
N	12,058	11,963	11,963	11,963

bonds (the analogue of a prospectus). The underwriter discount applies to all the CUSIPs in a given financing or "deal." (Recall that municipals are issued "in series," and each maturity has a separate CUSIP.).

The previous section showed that, despite being issued in larger deals with fewer CUSIPs per deal, the BAB are more underpriced than municipal bonds issued in the same period. To what extent is this a strategic response to the tax subsidy, which increases with underpricing, versus simply underestimation of the value of the bond or compensation to the intermediaries for the costs of distributing the bonds? The analysis in Section 2 suggests that underpricing increases the shared surplus to the issuers and underwriter, but the means through which the issuers would capture part of this surplus

Table 7
Determinants of investor markups. The table reports the determinants of markups on investor trades. Markups on investor trades are constructed as the mean markup over the reoffer price on all transactions not at the reoffer price, weighted by the trade par. The dependent variable is scaled by the reoffer price and expressed in basis points. Underpricing and characteristics are value weighted by bond deal. Specifications (A)–(C) are estimated using OLS. Specification (D) is estimated using a treatment effect model. All specifications include state fixed effects and month of issuance fixed effects. Standard errors are clustered by month of issuance. Levels of significance are denoted by * (5%), ** (1%), and *** (0.1%).

Determinant	(A)	(B)	(C)	(D)
BAB	78.98***	27.98***	26.23***	25.10***
Maturity		5.15***	5.27***	5.29***
Rating		2.06***	1.77***	1.75***
Unrated		2.83	2.40	2.30
Insured		17.04***	16.37***	16.33***
Callable		14.06***	13.87***	13.92***
Sinkable		6.23**	5.25**	5.30**
Fixed coupon		−3.90	−3.89	−3.88*
Extraordinary redemption		3.37	4.48	4.77
ln(deal size)		1.89	1.51	1.56*
Number of CUSIPs in deal		0.21	0.23*	0.21
Competitive offering		−39.44***	−39.57***	−39.60***
Refunding bond		2.22	1.72	1.59
Advanced refunding		0.75	0.67	0.61
Constant	75.33***	−0.33	−6.38	−6.23
State F.E.	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes
Issuer type F.E.			Yes	Yes
Use of proceeds F.E.			Yes	Yes
R^2	0.178	0.419	0.426	–
N	11,538	11,443	11,443	11,443

would be through lower fees, which are a transfer payment from the issuer to the underwriter. Accordingly, in this section we ask whether the gross spread is negatively correlated with the amount of underpricing.

Bonds that are simply distributed to institutional investors at or very near the reoffering price in large blocks should be relatively inexpensive to distribute, and in a competitive environment with no strategic incentive to underprice, we would expect underwriters to earn low fees on such deals. Placing bonds with retail investors is more costly, and underwriters can be compensated for those costs through fees or markup on the bonds. Market participants report that fees are more transparent to the press, the public, and regulators. Issuers and underwriters therefore are reluctant to raise fees, even when costs would justify doing so. Heterogeneity in costs, inventory risk, or dealer market power is more likely to be reflected in underpricing than in fees.

There is no definitive theoretical argument that would predict a particular cross-sectional relationship between fees and underpricing, since both could respond to underlying sources of heterogeneity. On the one hand, fixing the underwriters' total expected payoff, fees and underpricing clearly substitute for each other, which would lead to a negative association in the cross section. On the other hand, as costs rise both underpricing and fees may rise, leading to a positive cross-sectional correlation between the two.

Nevertheless, an outcome for BABs that contrasts with what we observe for tax-exempt bonds, or for taxable, non-BAB bonds, issued during the same period is suggestive of a strategic response to the tax incentives, particularly when controlling for the most obvious sources of heterogeneity in costs. This is what we find. Fees and underpricing are negative correlated for the BABs, but not for the tax-exempts. Moreover, this negative association tends to increase over the life of the BAB program. When compared to non-BAB taxable municipals the contrast is less striking, but still evident.

The average gross spread is lower for BABs than for tax-exempts (81.5 versus 92.8 basis points), and the deals are on average larger ($81 million par value versus $28 million). We would expect lower spreads for bigger deals if there are fixed costs to underwriting, as emphasized by Chen and Ritter (2000) in the context of IPOs. Table 8 reports coefficients from regressing average markups for bonds in each deal in our sample against controls along with the gross spread, a BAB dummy, and interaction terms. The first three columns, (A)–(C), pool together BABs and tax-exempt bonds. The last two columns pool BABs and non-BAB taxables. The first row shows BABs have higher markups over the reoffering price, as we have seen. The first specification, in column (A), shows that across all deals, both BABs and tax-exempts, the gross spread has a positive, though insignificant, correlation with the underpricing. The next column, (B), shows that when the spread is interacted with a BAB indicator, the markup is positively related to spread for tax-exempts, negatively for BABs. Column (C) shows that the interaction term becomes more negative through time. This suggests dealers and issuers may have taken some time to fully adjust to the incentives to underprice under the BAB program. For non-BAB taxables the association between spread and underpricing is negative but insignificant. It becomes significantly negative for BABs in the later periods of the life of the program.

Table 8
Gross spread and underpricing. The dependent variable is the average percentage markup over the reoffering price on all trades in a deal. The independent variable GS is the gross spread, BAB is an indicator for a BAB bond, and the intervals of months are indicators for the associated subperiod. The sample for columns A–C pools all deals on tax-exempt municipals and BABs. The sample for columns D and E pools BABs and non-BAB taxable municipals. Deal size is measured in hundreds of millions. The sample period from 05/2009 to 12/2010 is divided into five equal subperiods of four months each. Standard errors in parentheses are clustered by month of issuance. Levels of significance are denoted by * (5%), ** (1%), and *** (0.1%).

Determinant	(A)	(B)	(C)	(D)	(E)
BAB	98.59***	141.94**	139.52**	81.62*	85.40*
GS	0.11	0.50**	0.49****	−0.66	−0.66
BAB × GS		−5.19*	−1.45	−2.52	1.83
BAB × GS × (09–12/2009)			−3.34***		−0.66
BAB × GS × (01–04/2010)			−1.92***		−4.09***
BAB × GS × (05–08/2010)			−3.37***		−6.14***
BAB × GS × (09–12/2010)			−6.34***		−7.87***
Deal size	0.01	0.01	0.01	0.00	0.00
Number of CUSIPs in deal	1.16	1.22*	1.23*	−0.63	−0.50
Constant	34.41*	30.03*	14.46	158.14*	117.67*
State F.E.	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes
Issuer type F.E.	Yes	Yes	Yes	Yes	Yes
Use of proceeds F.E.	Yes	Yes	Yes	Yes	Yes
R^2	0.295	0.301	0.306	0.308	0.319
N	6151	6151	6151	1895	1895

6. Conclusion

The Build America Bond program was an attempt to give municipal issuers access to a deeper pool of investors. We have examined the effect this had on several measures of liquidity, and on the initial underpricing of the bonds when issued. The BABs are more underpriced than traditional tax-exempt municipal bonds issued in the same period. The prices between dealers show the most dramatic underpricing when compared to the tax-exempts. One reason for this might be that the tax rebate to the issuer on the bonds is tied to the coupon level, reducing the costs of underpricing to issuers, underwriters, and investors as a coalition.

While the BAB program may well have reduced financing costs for municipal entities, this does not appear to be for the reasons offered in support of the program since the bonds did not enjoy enhanced liquidity. Rather, the relative yields for the long-maturity taxable and tax-exempt bonds appear to reflect a lower marginal tax rate than the 35% rate used in the program.[17] In addition, the direct-pay mechanism substituted for the tax exemption of interest appears to have led to the unintended consequence of encouraging underpricing of the bonds.

Financial regulators have long been concerned with the costs and benefits of greater transparency in financial markets. Macro-economic policy makers are concerned with identifying mechanisms for fiscal intervention that minimize distortions or unintended transfers between actors who are not the target beneficiaries. The experience of the Build America Bond program illustrates how these concerns can interact. The municipal bond market was formerly completely opaque. Only in the last decade has it attained a basic level of post-trade transparency, allowing researchers, issuers, and investors to compare the prices issuers receive from underwriters to the prices at which the bonds eventually trade. Similarly, researchers, policy makers, and the public can now better assess the incidence, magnitude, and success of policy interventions through financial markets, and determine whether the financial markets are being used to subsidize activities other than those stated or intended. As these markets move towards greater pre-trade transparency, which is currently still lacking, prices can better serve their role of aggregating and communicating expectations about the future. This could, in principle, enable all parties to better coordinate policy, ensure capital flows efficiently, and encourage more competitive provision of financial services. It will be interesting to see if it does.

Acknowledgments

We greatly appreciate the help of Tal Heppenstall and John Roll who provided us with data and advice. Rich Ryffel of Edward Jones and Paul Luhmann of Stifle Financial patiently educated us about the process of issuing and trading municipal bonds. Of course, none of the above named individuals are responsible for any errors, omissions, or misinterpretations that, despite their best efforts, may have found their way into this paper. Participants in seminars at Carnegie Mellon, Notre Dame and Rice Universities, and at the 2012 UBC Summer Finance Conference and the Fall 2012 Carnegie-Rochester-NYU Conference provided helpful comments and suggestions. We thank our discussant, Erik Sirri and editors Marvin Goodfriend and Burton Hollifield, for a number of suggestions that focused and enhanced the analysis. The third author gratefully

[17] See Green (1993) for a theoretical explanation of the lower implicit rates at long maturities.

acknowledges research support from the Swiss Finance Institute and from NCCR FINRISK of the Swiss National Science Foundation.

Appendix A. Supplementary data

Supplementary data associated with this article can be found in the online version at http://dx.doi.org/10.1016/j.jmoneco.2013.04.010.

References

Ang, A., Bhansali, V., Xing, Y., 2010. Build America Bonds. Journal of Fixed Income 20, 67–73.

Ang, A., Bhansali, V., Xing, Y., 2011. Decomposing Municipal Bond Yields. Working Paper, Columbia University.

Ang, A., Green, R.C., 2011. Reducing Borrowing Costs for States and Municipalities through CommonMuni. Brookings Institute, Hamilton Project Discussion Paper 2011-01.

Chalmers, J., 1998. Default risk cannot explain the Muni Puxzzle: evidence from U.S.Government secured municipal bonds. Review of Financial Studies 11, 281–308.

Chen, H., Ritter, J.R., 2000. The seven percent solution. Journal of Finance 50, 1105–1131.

Green, R.C., 1993. A simple model of the taxable and tax-exempt yield curves. Review of Financial Studies 6, 233–264.

Green, R.C., 2007. Issuers, underwriter syndicates, and aftermarket transparency. Journal of Finance 62, 1529–1550.

Green, R.C., Hollifield, B., Schürhoff, N., 2007a. Financial intermediation and costs of trading in an opaque market. Review of Financial Studies 20, 275–314.

Green, R.C., Hollifield, B., Schürhoff, N., 2007b. Dealer intermediation and price behavior in the aftermarket for new bond issues. Journal of Financial Economics 86, 643–682.

Harris, L., Piwowar, M.S., 2006. Municipal bond liquidity. Journal of Finance 61, 1330–1366.

Hendershott, T., Madhavan, A., 2012. Click or call? Auction versus Search in the Over-the-counter Market. Working paper, University of California, Berkeley.

Schultz, P., 2001. Corporate bond trading costs: a peek behind the curtain. Journal of Finance 56, 677–698.

Schutz, P., 2012. The market for new issues of municipal bonds: the roles of transparency and limited access to retail investors. Journal of Financial Economics 106, 492–512.