Corruption, Political Connections, and Municipal Finance

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We show that state corruption and political connections have strong effects on municipal bond sales and underwriting. Higher state corruption is associated with greater credit risk and higher bond yields. Corrupt states can eliminate the corruption yield penalty by purchasing credit enhancements. Underwriting fees were significantly higher during an era when underwriters made political contributions to win underwriting business. This pay-to-play underwriting fee premium exists only for negotiated bid bonds where underwriting business can be allocated on the basis of political favoritism. Overall, our results show a strong impact of corruption and political connections on financial market outcomes. (*JEL* D73, G20, G22, G24, H74)

In this paper we examine how political integrity affects the interactions between governments, financial intermediaries, and financial markets. Political integrity reflects the absence of agency problems between elected or appointed government officials and their constituents; corruption in its various forms is the antithesis of political integrity. The bulk of theory and evidence indicates

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that corruption is costly because it impedes transactions, hinders trade, and retards financial and economic growth and development.[1]

We study how corruption and political connections affect primary security market transactions, and in particular, the issuance of municipal bonds. Specifically, using an intracountry empirical design, we exploit the distinctive features of the municipal bond underwriting market in the United States to identify the effects of political integrity on these primary financial market transactions.

We measure cross-sectional variation in (the inverse of) states' political integrity with a widely used proxy, namely, per capita federal corruption convictions (see, for example, Fisman and Gatti 2002; Fredricksson, List, and Millimet 2003; Depken and LaFountain 2006; Glaeser and Saks 2006).[2] This measure views states with low levels of per capita corruption convictions as having high political integrity, and vice versa. A separate and distinct source of variation in political integrity comes from abrupt changes in the extent to which municipal bond underwriting involves "pay-to-play" (discussed in more detail below). Pay-to-play is a practice whereby investment banks seek to win underwriting business by making campaign contributions to legislators who were involved in the process of selecting underwriters for municipal bond issuances in the state. We hypothesize that political integrity may have been lower during the pay-to-play era, that is, when legislators' decisions about municipal bond underwriting were exposed to financial persuasion from underwriters, as such exposure would create an increased potential for agency problems. We use these two sources of variation in political integrity—cross-state variation in corruption convictions and intertemporal variation due to pay-to-play—to examine the following four questions.

First, we ask whether political integrity affects credit risk.[3] We posit that extensive state corruption increases the likelihood of an issuer defaulting on

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[1] See, among others, Rose-Ackerman (1978), Shleifer and Vishny (1993), Mauro (1995), La Porta et al. (1999), and Wei (2000).

[2] We also use a proxy based on the stringency of anticorruption laws in the state. We discuss this in more detail below.

[3] How might state corruption affect the credit risk and in turn the costs of issuing municipal debt? Depken and LaFountain (2006) describe a *general* mechanism through which corruption can impact credit ratings, which we paraphrase as follows: in corrupt states, the costs of projects financed by municipal debt might be inflated if bureaucrats receive kick-backs, project selection is based on what is best for the corrupt decision maker and not what is best for the municipality or state, and general economic growth of the state can be slowed and/or debt capacity can be diminished if suboptimal projects are systematically chosen; at the margin, these effects can impact a state's credit ratings and borrowing costs. The size of such marginal effects is ultimately an empirical question, however. For a *specific* case of how corruption can affect repayments on municipal bonds, see Anderson v. Kutak, et al., 94-30126 (5th Cir. 21 April 1995). Briefly, the case involved collaboration among underwriters (Drexel Burnham Lambert, primarily), other financial institutions, and corrupt municipalities and government agencies with the authority to float bond offerings. In 1986, the municipalities issued $1.85 billion in bonds to fund housing and agricultural loans. The indenture trustee used the proceeds to buy guaranteed investment contracts (GICs) from a life insurance company with whom they had conspired. Investors were duped into thinking the GICs made the bonds very safe securities, but the proceeds of the GIC purchase were invested in junk bonds (this was not disclosed), thereby increasing the securities' default risk. To ensure that the insurance company retained full use of the proceeds, the issuers made it virtually impossible for potential borrowers to receive one of the home or agriculture loans that were ostensibly the *raison d'etre* of the bond issues. The bonds ultimately defaulted after the junk bond market crashed and the insurance company was placed into conservatorship.

their securities. Unlike macro-level studies of the determinants of credit risk (Butler and Fauver 2006; Butters, Depken, and LaFountain 2006; Depken and LaFountain 2006), we make use of bond-level data that allow us to examine contractual features that strengthen or attenuate the effects of political integrity. We find that less corruption is associated with better bond ratings, *ceteris paribus*: on average, a bond issued by a highly corrupt (top quartile) state has a significantly lower bond rating than that of a less corrupt state.

Second, we ask whether political integrity affects the pricing of the securities. Because we find that corruption affects credit risk, it is natural to expect that the market will price this risk into the securities' yields. Indeed, we find that corrupt states pay significantly higher yields to maturity on their municipal securities, *ceteris paribus*. Our estimate of the corruption penalty on yields is statistically different from zero and ranges from 6.6 to 10.4 basis points depending on the specification. Our estimates suggest that the corruption premium is roughly the equivalent of a bond issue being rated more than 2.0 notches lower (e.g., from A- to below BBB), *ceteris paribus*.

Interestingly, issuers appear to be able to separate out and sell the corruption component of a bond's overall risk. According to our empirical estimates, credit-enhanced bonds have no statistically significant corruption penalty. Thus, issuers are able to undo *completely* the negative effects of corruption on their bond yields by obtaining credit enhancements. Of course, this is not a free lunch—issuers who buy credit enhancements simply transfer the costs associated with paying a higher yield to paying a credit enhancement fee.[4] However, revealed preference suggests that the costs of corruption exceed the costs of credit enhancements: it is precisely these corrupt states that are most likely to purchase credit enhancements. One interpretation, and the one we favor, of this result is that credit enhancement is a channel through which corruption's effects can be attenuated, thereby suggesting an important role for financial institutions (i.e., institutions providing credit enhancements) in alleviating the economic damage that corruption can cause. A darker interpretation is that credit enhancement allows issuers to hide the economic damage of their corruption, with corruption costs being transferred from the relatively transparent interest rate to the relatively opaque credit enhancement fees. In section 4.5, we provide some indirect evidence in favor of the former interpretation.

Third, we ask whether political integrity affects the pricing of underwriting services.[5] We find that, in contrast to the effect of state corruption on yields, investment banks do not charge higher fees in corrupt states. However, we do find that the pay-to-play era had a strong impact on investment banking fees.

[4] We note that data on the cost of obtaining credit enhancements are, unfortunately, not available. Were these data available, they would provide an estimate of the shadow cost of corruption.

[5] Two recent articles in *The Wall Street Journal* (Richardson 2005; and Whitehouse 2005) discuss how corruption directly affects the municipal bond underwriting industry, including underwriters' hiring of highly paid, politically connected "consultants" to help raise "communities' awareness of (the underwriters and) their services and reputation."

During the pay-to-play era, when underwriting firms routinely made political campaign contributions to win underwriting business from the state, gross spreads were significantly higher, but *only* for negotiated bid deals, i.e., those deals that can be allocated on the basis of political favoritism. The effect is statistically significant and economically large—it ranges from 11.8 to 13.8 basis points, depending on the specification. This magnitude is roughly one-seventh of the mean gross spread. In contrast, competitive deals, which offer no room for favoritism, have fees that are only negligibly higher (and generally not statistically significant). This result continues to hold when controlling for underwriter fixed effects. We interpret these higher fees as the *quid pro quo* for political campaign contributions.

Fourth, we ask whether political integrity affects issuers' choice of financial institutions. As we discuss above, corrupt states are more likely to purchase credit enhancements, such as bond insurance or a letter of credit. We also find that issuers in corrupt states use lower quality underwriters. One explanation consistent with this result is that, at the margin, high reputation underwriters are unwilling to put their reputation on the line to underwrite bonds from corrupt states at prices that are competitive with less reputable underwriters.

Our paper makes contributions in several ways. First, while others document ways in which corruption affects the valuation of securities (Fisman 2001; Johnson and Mitton 2003; Khwaja and Mian 2005), we document how corruption affects securities' required returns. Second, we are the first to show the important role of financial intermediaries, specifically, credit-enhancing institutions, in allowing an issuer to shed the effects of corruption. Other papers document corruption's effects on government securities (e.g., Butters, Depken, and LaFountain 2006; Butler and Fauver 2006), but not what market participants might do about it. Third, we document a particular channel through which political connections can affect economic outcomes, specifically, investment bankers receiving *quid pro quo* by charging higher fees during the pay-to-play era.

Our results are robust to various perturbations of our tests and variables. These robustness checks show that it is unlikely that our results arise for more benign (and less interesting) reasons. For instance, political integrity is not simply proxying for other state-level macroeconomic effects because we find that its effects continue to hold when we control for a variety of state financial and economic conditions, such as state wealth (GSP per capita), size (population), economic health (number of business establishments), and financial health (interest coverage). We discuss the construction and inclusion of these additional control variables, and address a variety of selection, endogeneity, data, robustness, variable construction, and interpretation concerns in Section 4.7.

The remainder of the paper is organized as follows. Section 1 provides background information about the primary market for municipal securities, security underwriting, and the pay-to-play era. In Section 2, we discuss the data and methods we use. Section 3 discusses the characteristics of the data.

Section 4 presents our main multivariate results. Section 5 concludes and provides a discussion for the generality and applicability of our results.

1. Discussion of the Municipal Bond Market and the Pay-to-Play Era

The municipal bond (muni) market is inherently different from other new issues markets. In the case of munis, a state or local government, not a corporation, is the issuer, at least indirectly. The bonds typically mature in one to thirty years and fund public projects, such as roads, bridges, buildings, airports, and utilities. Every state has statutes that require "open meetings" or other disclosure of the terms of municipal bond offerings, and deal terms for negotiated muni offerings become public record before the bonds are actually issued. Furthermore, the Securities and Exchange Commission (SEC) has little power to directly regulate municipal bond issuers (Beckett 1997).

Munis generally fall into one of two categories: general obligation (GO) bonds and revenue bonds. GO bonds are backed by the full faith and credit of the issuing entity and are thereby guaranteed. There is usually a limit set on the amount of GO indebtedness an entity can issue at any one time. This limit is often referred to as the debt limit or debt cap. Revenue bonds do not carry the same guarantee as GO bonds do and are not typically limited by debt cap statutes. While GO bonds are usually paid from *ad valorem* revenues such as the general tax pool, revenue bonds are funded from specific fees, taxes, or assessments on the item they are supporting. For example, revenue bonds issued to fund a toll road might be repaid using the tolls collected on that road. GO bonds therefore carry lower interest rates because of the full faith and credit guarantee, whereas revenue bonds have higher rates since their repayment is dependent upon the success or failure of the project they support.

Before issuing either type of bond, the issuing entity, with the help of its financial advisor, must evaluate a few basic questions: how much money is needed to finance the project, what debt capacity is available, and what financial institutions and advisors will be used. The two most prevalent means of selecting investment bankers are through competitive bidding and negotiated contract. In competitive bidding, the governmental unit solicits and receives sealed bids. After receiving the sealed bids, the governmental unit opens them at a public hearing and reads aloud the deal terms submitted by each potential underwriter. Contracts are then awarded on the basis of the lowest bid received. In negotiated contract, a governmental entity first issues a Request for Proposal (RFP) or similar solicitation. Potential underwriters submit written proposals that are "graded" by the staff of the governmental unit. There may be oral presentations and question and answer sessions after the grading process or the government may award the contract on the basis of the proposals alone.

Historically, municipal bond underwriters have been notorious for bid rigging, bribery, insider trading, and other illegal activities (Mitchell and Vogel 1993). While recent regulatory scrutiny appears to have largely eliminated this sort of behavior, at one time personal and financial relationships between bond underwriters and politicians were a critical dimension of competition among rival investment banks. In order to get lucrative underwriting contracts, investment banks would routinely make substantial campaign and other political contributions to politicians who would allocate underwriting business to their municipality or state. This widespread practice became known as "pay-to-play," with these contributions considered a normal cost of doing business in the municipal underwriting industry. Some additional discussion of pay-to-play appears in Filling, Brozovsky, and Owsen (2002).

Intense scrutiny of the municipal bond market and pay-to-play practices began in 1993, shortly after Arthur Levitt became the Chairman of the SEC. The SEC brought nineteen municipal securities enforcement cases in the three years immediately following Levitt's appointment. Reform imminent, the municipal bond underwriting industry voluntarily agreed to cease making pay-to-play political contributions. In April 1994, the SEC established a rule that investment houses making political contributions could not sell bonds from that city/state for two years (Bradsher 1994). A suit was subsequently brought by William B. Blount, Chairman of the Democratic Party in Alabama and municipal banker at Blount Parrish Roton (a Montgomery, Alabama, investment bank). The (ultimately unsuccessful) suit argued that the SEC's stifling of pay-to-play was a violation of first and tenth amendment rights (Wayne 1994; and Gasparino 1998). The SEC's pressure had its intended effect and pay-to-play is no longer prevalent in municipal underwriting.

For additional institutional detail, we refer the interested reader to Nanda and Singh (2004) for a comprehensive discussion of bond insurance for municipals, and Harris and Piwowar (2006) and Green, Hollifield, and Schürhoff (2007) for additional general details on the municipal market, and on secondary market trading of municipal bonds.

2. Data, Methods, and Research Design

This section discusses the data and methods we use in the paper. We also provide succinct variable definitions in the Appendix.

2.1 Empirical proxies for corruption

Our primary (inverse) measure of a state's political integrity is the number of per capita corruption convictions of local, state, and federal officials during the sample period. These data are available from the U.S. Department of Justice's Public Integrity Section, and similar ex post measures of corruption have been widely used by Fisman and Gatti (2002); Fredricksson, List, and Millimet (2003); Glaeser and Saks (2006); and others. Glaeser and Saks (2006) provide a discussion of the convictions data (p. 1057):

> The crimes investigated by the Department of Justice (DOJ) include a wide array of topics such as conflict of interest, fraud, campaign-finance violations, and obstruction of justice. While the majority of public corruption cases are handled by the local U.S. attorney's office, the DOJ currently prosecutes about 2,000 cases per year. These cases are generally brought to the attention of the DOJ through four main channels. First, some cases are referred to the DOJ for federal prosecution if they involve individuals with close ties to local government, thereby making it inappropriate for them to be tried by the local U.S. attorney's office. The DOJ also handles cases that involve multiple jurisdictions. Third, federal agencies can directly refer questionable behavior of public employees to the DOJ for investigation. Finally, the DOJ can be called in to handle cases that require an unusual amount of resources or special supervisory assistance. According to the 2002 report, generally about half of the corruption convictions each year involve federal public officials.

We gather data on convictions for each state from 1990 through 2004. We define a state as corrupt during a given year if it falls within the top quartile of per capita state-year convictions. The results are qualitatively identical if we use just the number of convictions or a top tercile dummy instead.

We note that these data are for *convictions*; one might be interested in using indictments as an alternative proxy for corruption. Unfortunately, the DOJ makes indictment data available only at the national level, not at the state level, as we would need for our tests. In the aggregate, convictions are very highly correlated with charges filed in the same year. A simple time-series regression (not reported) of country-wide convictions on same year country-wide charges and charges lagged one, two, and/or three years shows that current charges are highly related to current convictions (the coefficient on current charges ranges from 0.72 to 0.77 depending on the lags and with t-statistics in excess of 6), but previous years' charges are not strongly related to convictions after controlling for current charges (the coefficients on the lagged charges variables are statistically insignificant and range from 0.06 to 0.11 depending on the specification). Although we cannot make any direct observation about whether the relation between convictions and charges is similar at the state level, the aggregate data suggest that convictions and indictments are closely related.

As an additional measure of corruption, we use a measure of the quality of state anticorruption laws. The Better Government Association (BGA), a Chicago, Illinois, based "civic watchdog" group, produces an Integrity Index based on the quality of states' laws regarding freedom of information, whistle blowing, campaign finance, gifts/trips/honoraria, and conflicts of interest disclosure. As described in the BGA Integrity Index report, the index "is a measure of the relative strength of existing laws that promote integrity in each of the fifty states. The [better] each state's score, the stronger its laws are and the better its citizens are protected (p. 2)." Thus, BGA ranks states based on the transparency, accountability, and limits imposed on government officials and bureaucrats. In our paper, we rank states based on the inverse of the BGA index scores so that a higher rank corresponds to inferior integrity of laws. This is so that all our measures of corruption are positively related, i.e., higher values of a measure imply higher corruption levels. Unlike the convictions measures for which we have a panel, we have only cross-sectional variation in the BGA index.

2.2 Other variables

We obtain data on municipal bond issues from the Securities Data Company's (SDC) Global Public Finance U.S. new issues database. We collect data on various bond characteristics for tax-free municipal bonds issued from 1990 through 2004, such as state issuing the bond, issue date, issue size, yield to maturity, investment banking gross spread (as a percentage of proceeds), years to maturity, underwriter identity, whether the lead underwriter is a minority-owned company, credit enhancement information, the type of bond (e.g., GO or revenue bond), method for selecting the underwriter (negotiated or competitive bid), and bond rating (or lack thereof). We have 127,976 observations, but some of our tests have fewer observations because some of the requisite data are unavailable. The number of observations used in each test is reported in the appropriate tables.

We quantify bond ratings by assigning numerical values, where higher numbers indicate higher credit quality. We assign a value of 21 to the highest rated bonds (Aaa or AAA), a value of 20 to the next-highest credit quality rating (Aa1 or AA+), and so on. This is the same procedure as that in Cantor and Packer (1997), except that they assign low numerical values to the highest rated bonds and high values to riskier bonds. Under our procedure, bonds with a rating of Ca3 or CC-, which are the lowest quality bonds in our sample, take the value of 0. State credit ratings are quantified in an analogous manner. We obtain Moody's state ratings from Texas Bond Review Board documents and Moody's historical ratings changes file. These include actual state ratings and ratings a state would have if it were to issue GO debt. Not all states have a rating.

Following common practice in the investment banking literature, we construct a proxy for investment bank reputation using the annual market share measure of Megginson and Weiss (1991).[6] We calculate market share using the total gross proceeds of the municipal bond offerings an investment bank manages in a year divided by the total gross proceeds of all municipal bond issuances in that year. We also construct a measure that we refer to as "matching treasury," which is the yield on a treasury security with the closest maturity to the bond. Finally, we gather data on both gross state product per capita and state-level tax rates on personal interest income for the highest tax bracket. Table 1 contains summary statistics on the variables of interest by state.

[6] We note that a common alternative, "tombstone rankings," based on Carter and Manaster (1990) and Carter, Dark, and Singh (1998), is not appropriate here because it is based on *equity* underwriting.

Table 1
Summary statistics

State	*Count*	*Convictions top quartile (%)*	*Convictions per millions population*	*BGA rank*	*State rating*	*Bond rating*	*No rating (%)*	*Yield (%)*	*Gross spread (%)*	*Credit enhancement (%)*	*Underwriter market share (%)*	*Size ($ millions)*	*Time to maturity (years)*	*GO bond (%)*	*Negotiated bid (%)*	*Minority (%)*
								Panel A								
Alabama	1,466	33.08	3.23	47	18.50	19.67	45.02	4.44	1.33	45.29	0.68	9.38	16.03	44.20	81.17	0.20
Alaska	155	27.10	4.11	23	19.00	20.54	16.77	4.58	0.92	73.55	3.45	36.58	13.14	58.06	76.77	0.65
Arizona	1,643	0.00	1.67	20		19.80	18.75	4.57	1.10	58.79	2.13	20.25	13.61	61.47	66.04	0.37
Arkansas	1,782	15.49	2.35	31	18.79	18.25	76.77	4.41	1.55	9.15	0.79	3.98	16.46	70.54	35.02	2.41
California	6,068	14.27	2.73	5	17.72	19.73	47.63	4.38	1.13	41.50	2.96	27.17	13.31	46.24	62.13	4.96
Colorado	1,419	6.55	1.49	16		19.53	32.98	4.52	1.05	48.06	1.55	16.12	14.18	46.93	81.54	0.14
Connecticut	1,832	14.03	2.33	13	18.65	19.69	32.04	4.31	0.67	29.53	2.36	18.76	12.20	91.21	32.81	0.82
Delaware	124	39.52	4.18	38	20.03	19.60	24.19	4.49	0.85	38.71	3.93	38.39	16.82	41.94	59.68	0.00
Florida	2,081	44.31	4.67	18	19.00	20.17	32.96	4.47	0.80	61.22	3.02	29.02	15.22	16.67	72.08	0.86
Georgia	1,311	26.85	3.14	26	21.00	19.08	22.35	4.38	0.76	46.68	1.83	27.72	15.42	31.05	68.80	0.61
Hawaii	125	48.00	4.43	4	18.34	20.13	12.00	4.79	0.69	63.20	7.36	103.75	18.62	69.60	96.00	0.00
Idaho	503	23.86	3.02	42	18.29	19.80	46.12	4.35	1.18	34.00	1.63	7.90	12.87	59.44	65.41	0.00
Illinois	7,438	70.02	4.95	41	18.69	19.86	50.54	4.52	1.16	33.10	0.63	8.34	11.08	83.54	61.23	0.74
Indiana	2,660	0.00	1.94	34	20.00	18.70	57.11	4.48	1.04	27.18	0.77	6.51	12.25	38.83	53.91	0.11
Iowa	4,451	0.00	1.28	43	20.00	18.81	65.47	4.41	1.27	14.92	0.65	2.92	10.98	69.22	45.05	0.02
Kansas	3,221	6.43	1.46	21	20.00	19.56	69.67	4.27	1.25	15.83	0.77	4.97	10.41	74.91	42.72	0.03
Kentucky	1,930	55.96	4.63	3	19.00	17.60	26.42	4.29	1.23	18.13	1.26	8.74	15.56	6.68	18.08	0.00
Louisiana	1,880	89.36	6.65	46	15.36	18.91	51.38	4.51	1.03	34.36	0.99	10.40	13.78	51.49	41.91	0.16
Maine	397	32.75	3.30	24	19.23	18.95	29.72	4.55	1.15	24.18	1.99	10.31	13.14	77.33	52.64	0.76
Maryland	695	15.83	2.17	10	21.00	19.46	17.41	4.49	0.73	29.64	3.36	46.99	17.50	57.70	34.96	0.86
Massachusetts	3,759	20.88	2.54	15	17.48	19.61	51.08	4.18	0.71	27.37	1.31	12.33	8.91	91.89	16.23	0.05
Michigan	4,942	5.93	2.09	32	18.98	19.15	46.46	4.40	1.09	28.05	1.00	6.62	13.95	70.58	27.84	0.42
Minnesota	7,230	0.00	1.18	17	20.03	17.88	49.76	4.39	1.29	11.74	0.91	5.23	11.00	81.84	25.50	0.08
Mississippi	1,413	74.66	6.55	33	18.48	18.18	50.25	4.47	1.11	21.44	0.79	7.87	15.28	68.93	34.47	1.63
Missouri	3,177	25.68	2.93	35	21.00	19.58	55.40	4.31	1.28	18.07	0.81	6.42	13.35	46.62	81.02	0.19
Montana	559	33.63	4.75	45	18.44	18.75	71.91	4.29	1.39	12.16	1.16	4.09	15.03	66.91	40.43	0.00
Nebraska	2,884	0.00	0.69	6		19.51	88.59	4.32	1.59	5.03	0.41	3.36	12.09	74.06	94.59	0.10
Nevada	537	7.26	1.83	30	19.00	19.80	21.23	4.37	0.90	50.28	3.06	23.04	12.95	70.39	25.14	0.56
N. Hampshire	352	0.00	1.02	36	19.06	19.46	33.24	4.40	0.86	33.52	2.15	12.26	13.31	80.40	33.81	0.00
New Jersey	6,057	35.84	3.55	12	19.80	20.05	46.82	4.00	0.76	35.78	1.71	11.35	9.48	86.53	22.12	1.40
New Mexico	943	0.00	1.74	48	19.77	18.83	28.42	4.37	1.03	33.40	1.75	12.04	11.66	68.08	31.50	0.00
New York	13,001	66.18	4.41	29	16.07	19.80	60.53	4.32	0.71	29.07	1.10	10.84	7.98	92.93	13.85	0.21

(continued overleaf)

Table 1
(Continued)

North Carolina	1,355	0.00	1.80	22	20.76	19.16	14.83	4.35	0.69	33.14	1.71	24.63	15.83	64.94	31.37	0.07
North Dakota	808	70.42	8.26	39	18.22	18.07	54.33	4.27	1.10	12.38	0.77	3.41	11.85	66.83	31.56	0.00
Ohio	6,938	46.07	4.30	14	19.48	19.22	79.26	4.15	0.81	14.04	0.65	6.83	6.36	82.21	72.12	0.27
Oklahoma	2,287	6.08	2.36	25	18.37	18.78	77.53	4.07	0.94	9.84	0.47	5.09	8.09	81.94	16.66	0.17
Oregon	1,431	0.00	0.73	19	18.87	19.53	49.76	4.21	0.91	27.67	1.97	12.11	11.41	61.01	71.49	0.00
Pennsylvania	4,360	45.80	4.02	40	18.03	20.35	18.56	4.00	0.82	74.33	1.25	12.09	13.71	69.84	72.80	0.78
Rhode Island	521	28.98	2.74	2	17.65	20.00	42.61	4.29	0.80	44.72	1.99	13.28	11.17	70.44	48.94	0.00
South Carolina	1,399	20.73	2.73	7	21.00	19.05	19.73	4.24	0.79	31.59	1.65	13.96	12.29	72.19	21.59	0.07
South Dakota	498	29.72	4.09	50		19.22	66.06	4.48	1.66	18.47	0.55	5.71	13.29	55.02	89.96	0.00
Tennessee	1,885	37.72	4.12	44	20.47	18.75	20.74	4.40	0.92	41.38	1.06	13.99	14.39	61.80	45.57	0.32
Texas	7,625	0.00	2.27	9	19.35	20.00	19.65	4.57	1.16	40.85	1.76	15.43	15.15	71.45	47.82	2.24
Utah	773	0.00	1.04	27	21.00	20.17	34.02	4.31	0.89	34.67	2.14	15.96	12.02	54.85	64.81	0.00
Vermont	124	14.52	1.87	49	19.31	18.95	20.97	4.43	0.86	28.23	3.93	14.99	15.05	71.77	39.52	0.00
Virginia	1,030	48.64	3.83	28	21.00	19.02	27.48	4.54	0.82	25.34	2.54	31.11	15.89	48.54	48.74	0.58
Washington	3,147	0.00	1.43	11	19.43	19.61	44.90	4.53	1.03	35.18	1.55	10.06	12.36	65.81	83.51	0.16
West Virginia	225	36.89	3.34	8	17.44	18.52	35.56	4.85	1.16	40.44	2.77	17.05	15.49	27.11	71.11	0.00
Wisconsin	7,296	0.00	1.58	1	18.71	19.08	44.81	4.33	1.20	24.56	0.70	5.81	10.29	81.48	49.34	0.07
Wyoming	239	28.45	3.11	37		18.86	47.28	4.53	1.04	17.57	0.87	7.12	10.82	38.49	60.25	0.00
								Panel B								
Mean of means		24.95	3.01	25	19.11	19.30	41.94	4.39	1.02	32.15	1.73	15.65	13.06	62.84	51.23	0.46
Mean	2,560	26.38	2.96	22	18.76	19.41	48.26	4.36	1.08	29.80	1.28	11.29	11.69	70.87	46.35	0.70
s.d.	2,688	44.07	2.16	14	1.63	2.24	49.97	1.25	0.64	45.74	2.62	43.22	7.65	45.44	49.87	8.34
Minimum	124	0.00	0.00	1	13.00	1.00	0.00	1.30	0.15	0.00	0.00	0.10	1.00	0.00	0.00	0.00
10th percentile	238	0.00	0.68	5	16.00	16.00	0.00	2.73	0.40	0.00	0.01	0.40	1.00	0.00	0.00	0.00
25th percentile	593	0.00	1.42	12	18.00	18.00	0.00	3.58	0.62	0.00	0.05	1.00	5.00	0.00	0.00	0.00
Median	1,555	0.00	2.59	20	19.00	21.00	0.00	4.38	0.96	0.00	0.24	3.00	11.42	100.00	0.00	0.00
75th percentile	3,210	100.00	4.26	33	20.00	21.00	100.00	5.07	1.45	100.00	0.89	7.91	18.68	100.00	100.00	0.00
90th percentile	6,967	100.00	5.66	41	21.00	21.00	100.00	6.00	2.00	100.00	3.96	21.51	20.27	100.00	100.00	0.00
Maximum	13,001	100.00	25.50	50	21.00	21.00	100.00	8.42	3.50	100.00	13.02	4, 671.52	30.00	100.00	100.00	100.00

This table presents summary statistics for pooled bond characteristics by state and for the overall sample. The variables are defined in the Appendix. The sample comprises 127,976 observations. In panel A, we report the number of bond issues (column heading "Count") and means of bond characteristics for each state. In panel B, we document various summary statistics for the overall sample. Specifically, under column heading count we report average bond state count and other distribution characteristics, and for the remaining variables we report state-weighted means (row header "Mean of means"), bond-weighted means (row header "Mean"), bond-weighted standard deviation (row header "s.d."), and other distribution characteristics as described in respective row headings.

2.3 Empirical methods

We have many bond issue observations for each state-year. Our corruption variables are measured at the state-year level (for convictions) or the state level (for the BGA Integrity Index), and do not vary separately at the bond level. We deal with this issue in two different ways. First, we cluster the standard errors by state-year to correct for this within-group correlation. This increases our standard errors relative to a nonclustered approach. (We note that with standard errors clustered by *state* rather than by state-year, the results are fundamentally the same, although in a few cases coefficients become marginally insignificant.) Second, we run regressions of state-year *means*. That is, we collapse the data to the mean value of each variable within each state-year. This procedure gives one observation for each state-year, for 750 maximum possible observations (50 states $\times$ 15 years $=$ 750 state-years). We could have fewer observations if there are no issues with complete data in a particular state-year. Because these means are measured with different precision depending on how many observations there are in the state-year, we perform weighted regressions where each mean is weighted by the number of observations that produced the value. When we want to study interactions, such as the interaction of convictions and credit enhancement or the interaction of pay-to-play and negotiated bid bonds, we collapse by state-year-type. The maximum possible number of observations for these tests is 1500 (50 states $\times$ 15 years $\times$ 2 types, where the two types are negotiated/competitive or credit enhanced/not enhanced).

3. Descriptive Statistics and Univariate Results

Table 1 provides a cross-sectional description of the municipal bond issues by state. There are a total of 127,976 bond issues, with the number of bond issues ranging from 124 each in Delaware and Vermont to 13,001 in New York. Nebraska has the lowest convictions per million population, with 0.69 compared to 8.26 for North Dakota. Glaeser and Saks (2006) report similar findings. The bond rating of the issues is roughly 19.5, which is equivalent to a Moody rating between Aa1 and Aa2. The overall average yield to maturity at the time of the bond issue is 4.39% (the average yield ranges from 4.04% for New Jersey to 4.85% for West Virginia). Because a bond's maturity and the overall level of interest rates may affect a bond's yield, we control for these variables in our regressions.

The average gross spread is 1.08% and nearly one-third of the bond issues have credit enhancements. The average underwriter market share for all of the bond deals is 1.28%. This percentage represents an underwriter's gross proceeds of their municipal bond offerings in a given year divided by the total gross proceeds of all municipal bond offerings in that year. Minority-owned underwriting firms are involved in an average of 0.70% of the offerings. The average bond offering matures in twelve years, with an average offer size of \$11.3 million. The smallest bond issues are from Iowa, with an average of $2.92 million, whereas Hawaii averages the largest amount, with $103.75 million.

Nearly one-half of the bond offerings are not rated and there are sizable differences in the dollar value of the offerings across states. The median bond size is $3 million, with a maximum of $4.67 billion. The yield to maturity on the bonds ranges from 1.3% to 8.4%. The gross spread charged by the underwriters is as low as 0.15%, but can be as high as 3.5%. This wide dispersion between yields and gross spreads represents a large cost disparity for the bond issues across states. Issues mature in one to thirty years.

4. Multivariate Results

In this section, we examine how political integrity affects bond characteristics, controlling for the factors known to affect bond issues, for example, offer size, maturity, type and structure of the bond offering, and economic conditions within each state. We use several multivariate regression models—using both issue-level tests and regressions on state-year means, as mentioned above—to identify whether political integrity affects bond ratings, yields, gross spreads, whether the issuing authority decides to issue credit enhancements with a given bond, and choice of underwriter. In each of our regressions, we focus on the high corruption indicator variable to measure corruption, but also report results for another measure: an indicator for the state being in the bottom quintile of quality of anticorruption laws (based on the BGA rank).

4.1 Determinants of bond ratings

Table 2 examines the determinants of bond ratings. Our interest is in how state corruption affects ratings. Aside from our corruption measures, our regressions have several control variables, namely, dummy variables for the use of minority-owned underwriters, whether the bond is a GO bond, and whether the bond is sold through a negotiated bid, as well as continuous variables for logged issue size, logged maturity, logged underwriter market share, Gross State Product per capita, and year dummies. We also include indicators for country regions, dividing the country into four parts as classified by the U.S. Census Bureau: West, Midwest, South, and Northeast. If the effects of corruption are driven by general demographics of a geographic region, our regional indicators should subsume the impact of the corruption variable. Each regression in the table includes all non-credit-enhanced bond issues in our sample with a bond rating. We exclude credit-enhanced bonds because their ratings are determined by the credit quality of the credit-enhancing body, not the bond issuer.

We present four regression models. The first two models use bond issues as the unit of observation; the last two models use state-year means. Because bond ratings are ordinal rather than continuous, we use an ordered logit model for our issue-level tests (we note that using a simple OLS specification gives similar results). For our regressions of means, we use OLS. The first and third

Table 2
Bond rating determinants

	Issue level				Regressions of means	
	Convictions top quartile		BGA rank ≥40		Convictions top quartile	BGA rank ≥40
	Coefficient	Elasticity	Coefficient	Elasticity	Coefficient	Coefficient
Corruption measure	−0.3540***	−0.0300	−0.2720***	−0.0233	−0.2647***	−0.4771***
	(0.000)		(0.001)		(0.003)	(0.000)
Minority indicator	−0.1508	−0.0131	−0.1709	−0.0147	−1.8542	−2.4788
	(0.326)		(0.272)		(0.333)	(0.206)
GO bond indicator	1.0932***	0.0832	1.1123***	0.0846	1.7729***	1.9713***
	(0.000)		(0.000)		(0.000)	(0.000)
Negotiated bid indicator	0.0143	0.0013	0.0164	0.0015	1.3281***	1.4862***
	(0.832)		(0.807)		(0.000)	(0.000)
Ln*(Size)*	0.5189***	0.0689	0.5229***	0.0696	0.3070***	0.3127***
	(0.000)		(0.000)		(0.001)	(0.000)
Ln*(Maturity)*	0.0688	0.0031	0.0494	0.0023	0.3696***	0.5261***
	(0.149)		(0.298)		(0.001)	(0.000)
Ln*(Underwriter market share)*	2.9434***	0.0081	2.8942***	0.0080	13.3061**	9.6318
	(0.000)		(0.000)		(0.031)	(0.106)
GSP per capita	0.0477***	0.0296	0.0456***	0.0284	0.0424***	0.0382***
	(0.000)		(0.000)		(0.000)	(0.001)
Year dummies	Yes		Yes		Yes	Yes
Region dummies	Yes		Yes		Yes	Yes
Pseudo-R^2 or R^2	0.079		0.078		0.599	0.611
Observations	30,259		30,259		740	740

This table presents regressions where the basic specification is $y = f(corruption, controls)$. We define all variables in the Appendix. The dependent variable, y, is bond rating. Column headers denote different corruption measures. We restrict the sample to issues without credit enhancement. The first two regressions use bond issues as the unit of observation, and are ordered logit regressions. For each ordered logit regression, we report coefficients and elasticities (which are the change in probability that the rating takes its highest value from a 1 standard deviation change in the independent variable around its mean, or from a 0 to 1 change if an indicator variable, while keeping all other variables constant at their means). The last two regressions involve a regression of state-year means, in which we compute the mean for each variable within each state-year, and use these means as the unit of observation in OLS regressions. We weight each mean by the number of observations used to compute it. Heteroskedasticity robust p-values (in parentheses) are computed based on standard errors that are adjusted for state-year clustering. *indicates coefficients that are significantly different from zero at a 90% confidence level; **significant at a 95% confidence level; ***significant at a 99% confidence level.

models use the top quartile of convictions dummy as the proxy for corruption. The second and fourth models replace the top quartile of convictions dummy with the BGA rank indicator that captures the efficacy of anticorruption laws in the state.

Corruption has a negative relation with bond ratings. This is consistent with corruption being viewed by rating agencies as a source of default risk. Specifically, based on our high corruption indicator variable, the ordered logit model shows that a bond issue from a corrupt state has a bond rating that is significantly lower relative to the same bond issue from a noncorrupt state. Issue-level OLS results (not tabulated) and regressions of means results indicate that the magnitude of this effect is between a quarter and a half rating notch. All else equal, a GO bond and a larger bond offering result in a higher bond rating. Other independent variables that affect bond ratings are underwriter market share (well-reputed underwriters are associated with better ratings), and gross state product per capita (wealthy states have better bond ratings, *ceteris paribus*).

4.2 Determinants of bond yields

Our main findings appear in Table 3, which reports the regressions with bond yield as the dependent variable. Aside from our corruption measures, we use several control variables that might also affect yields: indicator variables for credit enhancement, minority underwriter, negotiated bid, and GO bond, as well as logged issue size, the highest state income tax marginal tax rate, logged maturity, the yield on the U.S. treasury security with the closest maturity, logged underwriter market share, gross state product per capita, and a term that captures the interaction between corruption and credit enhancement.

We present seven regression models. The first four models use bond issues as the unit of observation; the last three models use means of each state-year group. Here the groups are credit-enhanced bonds and nonenhanced bonds. The first and fifth models are our baseline results; they use the top quartile of convictions dummy as the proxy for corruption.[7] The second model uses only observations that are not credit enhanced (and thus we omit the credit enhancement variable and its interaction with corruption). The third and sixth models add to our baseline model two more control variables: bond rating and an indicator for nonrated bonds. The fourth and seventh models replace the top quartile of convictions dummy with the BGA rank indicator that captures the efficacy of anticorruption laws in the state.

From these yield regressions, we conclude that corruption is associated with higher bond yields. In each model, the coefficient on our corruption measure is statistically significant. The coefficient on our corruption proxy is largest in the specifications that use the quality of anticorruption laws (that is, the BGA rank $\geq$40 indicator) as the measure of corruption—the coefficient is 9.5 basis points in the issue-level regression, and 10.4 basis points in the regression of means. The corruption coefficient is 6.7 basis points in the baseline issue-level regression, and 6.6 basis points in the baseline regression of means. In the restricted sample model with only bonds without credit enhancement, the coefficient on corruption is 5.9 basis points. (Although we do not report it in the table, we note that we obtain similar results with an underwriter fixed-effects model; that is, when an underwriter brings a bond to market for a corrupt issuer, the yield is significantly higher than if the same underwriter had brought to market a non-corrupt issuer's bond with the same characteristics. Likewise, we find similar results if we omit the most corrupt and least corrupt state.)

The impact of corruption on yields attenuates when we include as control variables the bond's rating and the nonrated indicator variable. This attenuation is not surprising—we are identifying in this specification a within-rating notch effect of corruption on yields. The coefficient on the corruption indicator

[7] We note that a continuous measure of convictions provides similar results.

Table 3
Yield determinants

	Issue level				Regressions of means		
	Convictions top quartile	*Convictions top quartile*	*Convictions top quartile*	*BGA rank ≥40*	*Convictions top quartile*	*Convictions top quartile*	*BGA rank ≥40*
Constant	2.3071***	2.4918***	2.4777***	2.2830***	1.2528***	1.0226***	1.3212***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)	(0.000)
Corruption measure	0.0668***	0.0587***	0.0510***	0.0952***	0.0658***	0.0553***	0.1039***
	(0.001)	(0.004)	(0.005)	(0.000)	(0.002)	(0.005)	(0.000)
Corruption × credit enhancement	−0.0613**		−0.0492*	−0.0635**	−0.0911***	−0.0855***	−0.1031***
	(0.031)		(0.072)	(0.042)	(0.001)	(0.001)	(0.002)
Credit enhancement indicator	−0.2666***		−0.0575***	−0.2707***	−0.2622***	−0.1403***	−0.2524***
	(0.000)		(0.000)	(0.000)	(0.000)	(0.002)	(0.000)
Minority indicator	−0.0244	−0.0203	−0.0033	−0.0215	−0.3991	−0.8561**	−0.3002
	(0.403)	(0.688)	(0.906)	(0.465)	(0.230)	(0.018)	(0.375)
GO bond indicator	−0.2149***	−0.3453***	−0.1610***	−0.2179***	−0.1258***	−0.0620	−0.1430***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)	(0.150)	(0.000)
Negotiated bid indicator	0.1354***	0.1952***	0.0943***	0.1323***	0.1117***	0.0403	0.0885***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.206)	(0.002)
Ln*(Size)*	−0.0491***	−0.0577***	−0.0083**	−0.0480***	−0.0141	0.0509***	−0.0099
	(0.000)	(0.000)	(0.011)	(0.000)	(0.327)	(0.004)	(0.481)
Ln*(Maturity)*	0.2230***	0.2102***	0.2881***	0.2192***	0.1892***	0.2532***	0.1557***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Ln*(Underwriter market share)*	0.3526**	−0.2886	0.5762***	0.3768**	0.5241	0.4209	0.7947
	(0.019)	(0.173)	(0.000)	(0.011)	(0.574)	(0.653)	(0.381)
Bond rating			−0.0336***			−0.0076	
			(0.000)			(0.659)	
No rating indicator			−0.2761***			0.2102	
			(0.000)			(0.542)	
Matching treasury	0.4347***	0.4394***	0.4343***	0.4347***	0.2305***	0.2303***	0.2334***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Tax	−0.0073***	−0.0111***	−0.0112***	−0.0066***	−0.0084***	−0.0120***	−0.0091***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
GSP per capita	0.0093***	0.0094***	0.0109***	0.0109***	0.0089***	0.0087***	0.0092***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-test	0.07	N/A	0.01	1.50	1.29	1.94	0.39
corruption + (*corruption* × *credit enhancement*) = 0	(0.797)		(0.932)	(0.221)	(0.255)	(0.163)	(0.535)
R-squared	0.748	0.754	0.759	0.748	0.966	0.967	0.966
Observations	88,588	57,586	88,588	88,588	1479	1479	1479

This table presents OLS regressions where the basic specification is $y = f($*corruption*, *corruption* × *credit enhancement*, *controls*). We define all variables in the Appendix. The dependent variable, y, is the bond *yield*. Column headers denote different corruption measures. The second column restricts the sample to issues without credit enhancement; all other columns are based on the full sample for which there are sufficient data. The first four regressions use bond issues as the unit of observation. The last three regressions involve a regression of state-year means, in which we compute the mean for each variable within each state-year separately for credit-enhanced and nonenhanced bonds; we use these means as the unit of observation in OLS regressions. We weight each mean by the number of observations used to compute it. Heteroskedasticity robust p-values (in parentheses) are computed based on standard errors that are adjusted for state-year clustering. *indicates coefficients that are significantly different at a 90% confidence level; **significant at a 95% confidence level; ***significant at a 99% confidence level.

becomes 5.1 basis points in the issue-level regression, and 5.5 basis points in the regression of means. The coefficient on bond rating is about 3.4 basis points, which provides a useful comparison for the magnitude of the corruption coefficient: depending on the specification, the corruption premium is roughly equivalent to a change of 1.5 to 3.1 ratings notches.[8]

There are several possible reasons that controlling for rating does not drive the corruption coefficient to zero. First, it could be that corruption is picking up within-rating variation in default risk, and that corrupt states are on average at the bottom of their rating class. Second, it could be that rating agencies are simply "getting it wrong" with their ratings, systematically rating bonds from corrupt states too high. Third, and the explanation that we favor, is that municipal bond ratings capture default probability, but not recovery rates (Moody's Investors Service 2007). More corrupt states might have lower expected recovery rates in the event of a default, and yields would reflect this but ratings would not.

The regressions show that credit-enhanced bonds have lower yields (the coefficients are all negative and statistically significant). More interesting than the direct effect of credit enhancement is how it *interacts* with corruption. We find that the corruption premium completely disappears for credit-enhanced bonds. For each of the regression models, the coefficient on the interaction term is about the same in absolute magnitude as the coefficient on the direct effect of corruption, and F-tests show that the sum of the two coefficients is indistinguishable from 0 in each specification, with p-values that fall well outside of traditional confidence values. This implies that third-party certification can alleviate the risks associated with corruption.

Other control variables generally behave as expected. Negotiated bids have higher bond yields. This accounts for the fact that these deals are more complicated and are likely to be riskier offerings. Not surprisingly, GO bonds (which are backed by state taxes) have lower yields. Larger bond offerings, a higher personal tax rate, and higher bond ratings also reduce the bond yield. A nonrated bond issue has a negative and marginally significant effect on yields. Bond yields are higher when the time to maturity and matching treasury yields are greater. Minority involvement and underwriter quality have no systematic effect on bond yields, although underwriter quality occasionally loads positively and significantly in our tests. We discuss some robustness tests in Section 4.7.

4.3 Determinants of underwriter gross spreads

Table 4 examines the determinants of underwriter gross spreads. Our regressors are our corruption indicator and other control variables: indicator variables for credit enhancement, minority-owned underwriter, GO bonds, negotiated bid, nonrated bonds, as well as bond rating, logged issue size, state tax rate, logged

[8] For example, if we consider the first specification, corruption increases yields by 7 basis points. Given that a one notch increase in ratings reduces yields by 3.4 basis points, corruption has an effect of 2 (= 7/3.4) rating notches.

Table 4
Gross spread determinants

	Issue level			Regressions of means	
	Year dummies	*Pay-to-play*	*Underwriter FE*	*Year dummies*	*Pay-to-play*
Constant	0.2648***	0.2022**	0.4668***	−1.5161***	−1.3400***
	(0.003)	(0.011)	(0.000)	(0.001)	(0.002)
Convictions top quartile indicator	−0.0193	−0.0247	−0.0027	−0.0182	−0.0335
	(0.421)	(0.345)	(0.869)	(0.622)	(0.384)
Corruption × *credit enhancement*	−0.0130	−0.0070	−0.0010	−0.0279	0.0100
	(0.640)	(0.812)	(0.961)	(0.684)	(0.889)
Credit enhancement	−0.0204	−0.0216	0.0130	−0.2139***	−0.1853**
	(0.225)	(0.209)	(0.342)	(0.005)	(0.016)
Minority indicator	−0.1152***	−0.1178***	−0.0099	0.5806*	0.6876**
	(0.000)	(0.000)	(0.944)	(0.076)	(0.030)
GO bond indicator	−0.0287**	−0.0283**	−0.0362***	−0.0273	−0.0182
	(0.018)	(0.018)	(0.000)	(0.561)	(0.695)
Negotiated bid indicator	0.2347***	0.1799***	0.0994***	0.1848***	0.1460***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Ln*(Size)*	−0.1481***	−0.1487***	−0.1330***	−0.1684***	−0.1725***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Ln*(Maturity)*	0.3889***	0.3875***	0.3495***	0.5094***	0.4966***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Ln*(Underwriter market share)*	−1.4302***	−1.4223***	0.2803	−2.2766**	−1.9294*
	(0.000)	(0.000)	(0.497)	(0.021)	(0.055)
Bond rating (rating for all)	0.0023	0.0010	−0.0080**	0.0765***	0.0605***
	(0.502)	(0.786)	(0.019)	(0.001)	(0.009)
No rating indicator	0.3128***	0.2882***	0.0838	1.8463***	1.5311***
	(0.000)	(0.000)	(0.138)	(0.000)	(0.000)
Tax	0.0028	0.0030	−0.0016	0.0098***	0.0093***
	(0.314)	(0.273)	(0.388)	(0.001)	(0.003)
GSP per capita	0.0000	0.0003	0.0005	0.0031	0.0029
	(0.995)	(0.890)	(0.711)	(0.211)	(0.249)
Pay-to-play indicator		0.0071	0.0026		0.0361
		(0.786)	(0.902)		(0.265)
Pay-to-play × *negotiated bid indicator*		0.1384***	0.1324***		0.1181***
		(0.000)	(0.000)		(0.000)
Time trend		−0.0149***	−0.0194***		−0.0195***
		(0.000)	(0.000)		(0.000)
Underwriter fixed effects	No	No	Yes	No	No
Year dummies	Yes	No	No	Yes	No
Region dummies	Yes	Yes	Yes	Yes	Yes
R-squared	0.357	0.360	0.456	0.668	0.664
Observations	40,514	39,764	39,764	1393	1393

This table presents OLS regressions where the basic specification is $y = f($*corruption*, *corruption* × *credit enhancement*, *pay-to-play*, *pay-to-play* × *negotiated bid*, *controls*$)$. We define all variables in the Appendix. The dependent variable, y, is the bond underwriting *gross spread*. Column headers denote different fixed effects; each regression contains region dummies. The first three regressions use bond issues as the unit of observation. The last two regressions involve a regression of state-year means, in which we compute the mean for each variable within each state-year separately for negotiated bid and competitive bid bonds; we use these means as the unit of observation in OLS regressions. We weight each mean by the number of observations used to compute it. When using the pay-to-play indicator with issue-level tests, we omit observations in the months immediately surrounding April 1994 (i.e., 31 March through 1 July 1994). Heteroskedasticity robust p-values (in parentheses) are computed based on standard errors that are adjusted for state-year clustering. *indicates coefficients that are significantly different at a 90% confidence level; **significant at a 95% confidence level; ***significant at a 99% confidence level.

maturity, logged underwriter market share, gross state product per capita, a variable that interacts corruption and credit enhancement, and year and region dummies.

We find no evidence that corruption has any systematic impact on investment banking fees. This nonresult is very robust, and holds for each of our corruption measures. Therefore, for parsimony we report only the results based on our top quartile of convictions indicator.

We present five models in Table 4. The first three models use bond issues as the unit of observation; the last two models use means of each state-year group. Here the groups are negotiated bid bonds and competitive bid bonds. The first and fourth models use year dummies and are useful as baseline models for observing the lack of effect that corruption has on underwriting gross spreads. The third model is an underwriter fixed-effects regression.

The second, third, and fifth models omit the year dummies and add an indicator variable for the pay-to-play era, an interaction of pay-to-play with negotiated bid bonds, and a linear time trend to absorb the general downward trend in underwriting fees. We discuss the pay-to-play results separately in the next section.

We observe that a larger offer size, GO bonds, and underwriter quality are associated with lower gross spreads. Negotiated bids, nonrated bonds, and increased time to maturity increase gross spreads. Negotiated bids are arguably more complicated, which could cause underwriters to charge more for their services. Minority underwritten bonds appear to have higher spreads when we use state-year means and lower spreads when we use issue-level tests. Because so few bonds have minority underwriters (less than 1%), we are reluctant to draw economic conclusions from this change.

4.4 Underwriter fees and the effect of pay-to-play

During the pay-to-play era, underwriters competed for underwriting mandates by making political campaign contributions to legislators who might influence the allocation of underwriting jobs. These campaign contributions could cause distortions in at least two different ways. First, they could change the allocation of contracts to underwriters with political connections. (Goldman, Rocholl, and So 2007 document such a distortion in the allocations of government procurement contracts to S&P500 firms.) Second, and more directly, campaign contributions might generate a *quid pro quo* in the form of higher fees for underwriting services. Although we cannot observe to whom underwriting contracts "should" have been allocated in the absence of pay-to-play, our data are well suited to address the second type of distortion.

Our hypothesis is that the pay-to-play era should have higher gross spreads, all else equal, as a *quid pro quo* to the underwriters. To test this conjecture, in the second, third, and fifth columns of Table 4, we regress gross spreads on our usual control variables, replacing our vector of year dummies with a pay-to-play era indicator and a linear time trend. Thus, the pay-to-play indicator identifies if there is a significant shift in the mean underwriting gross spread around pay-to-play after controlling for any secular trends and a host of control variables.

We do not expect pay-to-play effects to be equally distributed among all bonds. Specifically, we hypothesize that any *quid pro quo* would likely come from negotiated bid deals, not competitive bid deals, because the former can be allocated on the basis of political favoritism but the latter cannot. To examine this, we interact the pay-to-play indicator with the negotiated bid indicator.

We find that, overall, during pay-to-play gross spreads were significantly higher, but *only* for negotiated bid deals. The coefficient on the pay-to-play indicator is negligible, which reflects the average change for competitive bid deals. However, the coefficient on the interaction between pay-to-play and negotiated bids is a statistically significant 13.8 basis points (13.2 basis points with underwriter fixed effects; 11.8 basis points in our regression of means test). This means that, other things equal, negotiated bid deals had underwriter gross spreads of 12–14 basis points (about one-seventh of the mean gross spread) higher during the pay-to-play era, but there was no meaningful difference in gross spreads for the competitive bids that could not be allocated on the basis of political favoritism. We discuss some robustness tests in Section 4.7.

4.5 Determinants of the credit enhancement choice

The regressions presented so far indicate that credit enhancements have a significant effect on the yields and the corruption yield penalty in the municipal bond market. In this section, we examine the determinants of the choice to purchase credit enhancement. We run logistic regressions with credit enhancement as the dependent variable to determine what leads issuers to purchase credit enhancements. For our regressions of means, we use OLS. In Table 5 we report the regressions. For each of our specifications—issue level and regressions of means, using top convictions quartile, or the indicator for poor anticorruption laws (BGA rank $\geq$40)—we find that corrupt states are more likely to use credit enhancements.

There are two possible (not mutually exclusive) interpretations for these results. States that are corrupt pay a premium on their bonds due to the increased risk that bondholders face. These states are most likely to benefit from purchasing credit insurance. Of course, the rates that credit-enhancing institutions charge for bond insurance or other enhancements should be higher for exactly these corrupt states. The fact that corrupt states express a revealed preference for purchasing credit enhancements strongly suggests that the benefits from purchasing such enhancements outweigh the costs. If these benefits are *public*, and accrue to the issuing state, then the revealed preference argument suggests that credit enhancement is a net benefit and a solution to the costs that corruption creates. This net benefit from credit enhancement could arise if credit enhancers are information-producing specialists. For instance, bond insurers may serve as delegated monitors of municipal issuers: they insure many issues

Table 5
Credit enhancement determinants

	Issue level				Regression of means	
	Convictions top quartile		*BGA rank ≥40*		*Convictions top quartile*	*BGA rank ≥40*
	Coefficient	Elasticity	Coefficient	Elasticity	Coefficient	Coefficient
Constant	−5.7617***		−6.0956***		−0.2420***	−0.1687**
	(0.000)		(0.000)		(0.001)	(0.019)
Corruption measure	0.1787**	0.0298	0.5361***	0.0948	0.0253**	0.0768***
	(0.010)		(0.000)		(0.017)	(0.000)
Minority indicator	0.0454	0.0075	0.0720	0.0118	0.1735	0.2119
	(0.712)		(0.559)		(0.587)	(0.502)
GO bond indicator	−0.0188	−0.0031	−0.0485	−0.0079	0.0898***	0.0569**
	(0.709)		(0.328)		(0.001)	(0.039)
Negotiated bid indicator	0.3827***	0.0627	0.3474***	0.0563	0.1486***	0.1230***
	(0.000)		(0.000)		(0.000)	(0.000)
Ln*(Size)*	0.4660***	0.1161	0.4672***	0.1153	0.1655***	0.1599***
	(0.000)		(0.000)		(0.000)	(0.000)
Ln*(Maturity)*	1.4389***	0.2023	1.4332***	0.1996	0.1118***	0.0767***
	(0.000)		(0.000)		(0.000)	(0.000)
Ln*(Underwriter market share)*	−4.3049***	−0.0175	−3.9765***	−0.0160	−2.2132**	−1.0031
	(0.000)		(0.000)		(0.033)	(0.314)
GSP per capita	0.0007	0.0008	0.0160*	0.0177	−0.0038**	−0.0028*
	(0.940)		(0.053)		(0.022)	(0.064)
Year dummies	Yes		Yes		Yes	Yes
Region dummies	Yes		Yes		Yes	Yes
Pseudo-R^2	0.283		0.287		0.754	0.771
No. of observations	127,974		127,974		750	750

This table presents regressions where the basic specification is $y = f(corruption, controls)$. We define all variables in the Appendix. The dependent variable, y, is the *credit enhancement* indicator. Column headers denote different corruption measures. The first two regressions use bond issues as the unit of observation, and are logistic regressions. For each logistic regression, we report coefficients and elasticities (which are the change in probability from a 1 standard deviation change in the independent variable around its mean, or from a 0 to 1 change if an indicator variable, while keeping all other variables constant at their means). The last two regressions involve a regression of state-year means, in which we compute the mean for each variable within each state-year, and use these means as the unit of observation in OLS regressions. We weight each mean by the number of observations used to compute it. Heteroskedasticity robust p-values (in parentheses) are computed based on standard errors that are adjusted for state-year clustering. *indicates coefficients that are significantly different at a 90% confidence level; **significant at a 95% confidence level; ***significant at a 99% confidence level.

for the same issuers, but bond purchasers may have trouble committing to monitor issuers due to free rider problems. The repeat transactions could reduce monitoring costs at the margin. This intuition is similar to that of the Diamond (1984) model of delegated monitoring of borrowers by banks.

On the other hand, if the benefits from purchasing credit enhancement are primarily *private* benefits that accrue to the corrupt decision makers in the issuing state, then corruption distorts the credit enhancement choice and actually enables corruption to persist. If institutions providing credit enhancement can price discriminate, they might even charge disproportionately higher fees to issuers from corrupt states, thereby exacerbating, rather than mitigating, corruption's effects.

These two interpretations are difficult to distinguish with available data, particularly because credit enhancement fees are not readily available. Ideally, one would want to compare (a) the difference in credit enhancement fees for highly corrupt issuers versus less corrupt issuers to (b) the present value of savings from avoiding the corruption premium that would be reflected in higher bond yields. We cannot compute (a) with our data, but we can estimate (b).

We estimate the present value of savings from avoiding the corruption premium that would be reflected in higher bond yields with our coefficient estimate from the first regression specification in Table 3. For corrupt states' bond issues, we compute the present value of the corruption premium that is (for credit-enhanced bonds) or would be (for non-credit-enhanced bonds) eliminated by using credit enhancement—6.68 basis points per year. We use each bond's maturity as the number of periods in an annuity and the bond's actual yield as a discount rate. We estimate the average present value of the savings to be 62.7 basis points. (We note that using our corruption premium estimate from our two-stage least-squares (2SLS) results in Table 7, the magnitude goes to 232 basis points.) While we cannot directly compare this estimate of the benefits to the difference in credit enhancement fees for highly corrupt issuers versus less corrupt issuers (or even an actual figure of total credit enhancement fees), we do know that, in general, the average total bond insurance premium is about 50 basis points (see Nanda and Singh 2004). Therefore, it seems likely that the 62.7 basis point present value saving could exceed the *difference* in credit enhancement fees for highly corrupt issuers versus less corrupt issuers. If so, it suggests that at least some net benefit from credit enhancement accrues to the public. However, we cannot rule out that private benefits do not accrue to the corrupt decision makers, nor can we say that such private benefits distort the credit enhancement decision.

4.6 Determinants of underwriter choice

In Table 6, we examine the determinants of issuers' choice of underwriter reputation. As with our other tables, we present results for both issue-level regressions (first three models) and regressions of means (last three models), using either our top quartile of convictions measure of corruption (the first, second, fourth, and fifth models) or our measure based on the anticorruption laws (the BGA rank $\geq$40 measure; third and sixth columns). Following the previous literature on underwriter reputation, we use the underwriter's market share in a given year as our proxy for reputation. (Although we do not tabulate the results, we note that our results also hold if we measure underwriter reputation based on annual market share in the *corporate* debt underwriting market.) Models presented in the second and fifth columns include a pay-to-play indicator and time trend.

In all of the regressions, we observe that more corrupt states use lower reputation underwriters on average, holding other factors constant. The regressions also show that minority-owned investment banks have lower market share. GO bonds and negotiated bids use lower quality underwriters. Larger and longer maturity issues, and issues from wealthier states, use better underwriters. We

Table 6
Choice of underwriter

	Issue level			Regressions of means		
	Convictions top quartile	*Convictions top quartile*	*BGA rank ≥40*	*Convictions top quartile*	*Convictions top quartile*	*BGA rank ≥40*
Constant	0.0015	−0.0070***	0.0026*	0.0081***	0.0010	0.0047*
	(0.313)	(0.000)	(0.062)	(0.003)	(0.737)	(0.065)
Corruption measure	−0.0011***	−0.0011***	−0.0030***	−0.0013***	−0.0014***	−0.0028***
	(0.007)	(0.008)	(0.000)	(0.001)	(0.001)	(0.000)
Minority indicator	−0.0174***	−0.0174***	−0.0175***	0.0430***	0.0469***	0.0394***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.003)
GO bond	−0.0029***	−0.0029***	−0.0028***	−0.0047***	−0.0041***	−0.0031**
	(0.000)	(0.000)	(0.000)	(0.001)	(0.004)	(0.021)
Negotiated bid	−0.0029***	−0.0029***	−0.0027***	−0.0038***	−0.0036***	−0.0028***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)
Ln*(Size)*	0.0064***	0.0064***	0.0064***	0.0075***	0.0075***	0.0074***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Ln*(Maturity)*	0.0014***	0.0014***	0.0016***	−0.0006	−0.0009	0.0008
	(0.000)	(0.000)	(0.000)	(0.435)	(0.282)	(0.313)
GSP per capita	0.0004***	0.0004***	0.0003***	0.0002***	0.0002***	0.0002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Pay-to-play indicator		0.0019***			0.0014**	
		(0.001)			(0.012)	
Time trend		0.0000			0.0001	
		(0.879)			(0.225)	
Year dummies	Yes	No	Yes	Yes	No	Yes
Region dummies	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.222	0.218	0.221	0.752	0.741	0.762
No. of observations	127,974	125,676	127,974	750	750	750

This table presents OLS regressions where the basic specification is $y = f(corruption, controls)$. We define all variables in the Appendix. The dependent variable, y, is the natural logarithm of *underwriter market share.* Column headers denote different corruption measures. The first three regressions use bond issues as the unit of observation; the last three regressions involve a regression of state-year means, in which we compute the mean for each variable within each state-year, and use these means as the unit of observation in OLS regressions. We weight each mean by the number of observations used to compute it. When using the pay-to-play indicator with issue-level tests, we omit observations in the months immediately surrounding April 1994 (i.e., 31 March through 1 July 1994). Heteroskedasticity robust p-values (in parentheses) are computed based on standard errors that are adjusted for state-year clustering. *indicates coefficients that are significantly different at a 90% confidence level; **significant at a 95% confidence level; ***significant at a 99% confidence level.

find that during pay-to-play, issuers used underwriters with higher market share. (We note that when we add a term that captures the interaction between pay-to-play and the corrupt state indicator to examine whether underwriters engaged in pay-to-play activities more aggressively in corrupt states, we find no significant relation.) One interpretation of this result is that larger investment banks used their deep pockets and clout to win underwriting business during the pay-to-play era.

Why do corrupt states use less reputable underwriters? Such states are precisely the states that might *prefer* a more reputable underwriter to certify their issues, *ceteris paribus*. The choice of a less reputable underwriter might be because corrupt states allocate some underwriting business to underwriting firms with political connections that arise independently of their pay-to-play contributions. Another possibility, and the one we favor, is as follows. High reputation underwriters are reluctant to put their reputation at stake underwriting

Table 7
Yield determinants: two-stage least squares

	Corruption measure: *Convictions top quartile*			Corruption measure: *BGA rank ≥40*		
	First stages		Second stage	First stages		Second stage
Dependent variable:	*Credit enhancement*	*Corruption × Credit enhancement*	*Yield*	*Credit enhancement*	*Corruption × Credit enhancement*	*Yield*
	Panel A: Issue level					
Corruption measure	0.2678*	0.8832***	0.2563***	−0.4457***	0.3290***	0.3574***
	(0.081)	(0.000)	(0.000)	(0.005)	(0.008)	(0.000)
Instrumented: *Corruption × credit enhancement*			−0.4435***			−0.5983***
			(0.004)			(0.000)
Instrumented: *Credit enhancement*			−0.1050			−0.0037
			(0.170)			(0.966)
State credit rating	−0.0214***	−0.0005		−0.0322***	0.0007	
	(0.000)	(0.743)		(0.000)	(0.330)	
State credit rating × Corruption	−0.0134*	−0.0404***		0.0186**	−0.0138**	
	(0.090)	(0.000)		(0.021)	(0.041)	
State % bonds with credit enhancement last year	0.6026***	−0.0868***		0.4338***	−0.0663***	
	(0.000)	(0.000)		(0.000)	(0.001)	
% bonds with credit enhancement last year × corruption	0.0933	0.9165***		0.3466***	0.9860***	
	(0.147)	(0.000)		(0.000)	(0.000)	
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
F-test: *Corruption* + (*corruption × credit enhancement*) = 0			4.27**			7.43***
			(0.039)			(0.007)
R-squared	0.260	0.525	0.649	0.261	0.576	0.643
Observations	78,756	78,756	78,756	78,756	78,756	78,756
	Panel B: Regressions of means					
Corruption measure	−0.0688	0.6931***	0.1974**	−1.2295***	0.1643*	0.3866***
	(0.801)	(0.000)	(0.012)	(0.000)	(0.071)	(0.000)
Instrumented: *Corruption × credit enhancement*			−0.4937**			−0.7771***
			(0.012)			(0.000)
Instrumented: *Credit enhancement*			0.4847***			0.2413**
			(0.004)			(0.019)
State credit rating	−0.0488***	−0.0053**		−0.0626***	−0.0034	
	(0.000)	(0.022)		(0.000)	(0.103)	
State credit rating × corruption	0.0009	−0.0313***		0.0533***	−0.0057	
	(0.951)	(0.000)		(0.000)	(0.229)	
State % bonds with credit enhancement last year	−0.3063***	−0.3488***		−0.5841***	−0.2989***	
	(0.003)	(0.000)		(0.000)	(0.000)	
% with credit enhancement × corruption	0.2596*	0.9343***		0.7006***	0.9766***	
	(0.057)	(0.000)		(0.000)	(0.000)	
Other controls	Yes	Yes	Yes	Yes	Yes	Yes

(continued overleaf)

Table 7
(Continued)

F-test: *Corruption* + (*corruption* × *credit enhancement*) = 0			5.57** (0.019)			18.39*** (0.000)
R-squared	0.623	0.577	0.895	0.634	0.582	0.927
Observations	1141	1141	1141	1141	1141	1141

This table presents two-stage least-squares regressions, allowing credit enhancement to be endogenous. First-stage equations separately estimate two endogenous variables: *credit enhancement* and the interaction *credit enhancement* × *corruption*. The basic specification for the second stage is $y = f$(*corruption*, *corruption* × *credit enhancement (instrumented)*, *credit enhancement (instrumented)*, *controls*). The dependent variable in the second-stage equation, y, is the bond *yield*. Although we include the following vector of control variables in all of the regressions, we do not report their coefficients in the table to conserve space; the control variables are *Minority*, *GO bond*, *Negotiated bid,* Ln*(Size),* Ln*(Maturity),* Ln*(Underwriter market share), Matching treasury, Tax, GSP per capita, Year dummies, Region dummies*, and an intercept. We define all variables in the Appendix. Column headers denote different corruption measures. Column subheadings denote which dependent variable estimation is presented. Our instrumental variables (IVs) are *state credit rating*, *state credit rating* × *corruption*, *state% bonds with credit enhancement last year*, and *state% bonds with credit enhancement last year* × *corruption*. Our instrumenting strategy reduces our sample size by requiring states to have ratings, and by using our first year of data to produce the *state% bonds with credit enhancement last year* instrument. Panel A presents the regressions that use bond issues as the unit of observation. Panel B presents regressions of state-year means, in which we compute the mean for each variable within each state-year separately for credit-enhanced and nonenhanced bonds; we use these means as the unit of observation in each of the regressions. We weight each mean by the number of observations used to compute it. Heteroskedasticity robust p-values (in parentheses) are computed based on standard errors that are adjusted for state-year clustering. Second stage standard errors are corrected for the bias inherent in two-stage least-squares estimators. *indicates coefficients that are significantly different at a 90% confidence level; **significant at a 95% confidence level; ***significant at a 99% confidence level.

securities from corrupt states, *ceteris paribus*. To be willing to offer their services to corrupt states, high reputation underwriters charge higher rates, and corrupt states react by using lower quality underwriters.

4.7 Robustness and other empirical considerations

Empirical tests such as the ones in this paper face a number of econometric challenges. We discuss some of these challenges and robustness tests in this subsection. We begin with broad interpretation issues, then discuss how our results stand up to alternative econometric methods, and finish the section with a discussion of additional control variables to capture state-specific economic and financial conditions.

4.7.1 Sample selection. Because corrupt states might be less likely to issue securities due to the increased costs they face, sample selection bias may be a concern. This bias would work against finding our results. If corrupt states were choosing not to issue (or, put differently, if corrupt issuers were only choosing to issue when they receive abnormally good deals), we would find a muted relation between corruption and issuance characteristics, such as yields and ratings. We do in fact observe that the state-year volume of bonds issued per capita is smaller in number and dollar value in more corrupt states (results not tabulated). Thus, our results are conservative estimates of the impact of corruption.

4.7.2 What do the convictions data mean? One potential criticism of using the convictions data as the basis for our measure of corruption is that a large number of convictions might indicate a lot of corrupt activity, or it might indicate aggressive enforcement (and hence low corruption because all the corrupt individuals get caught). We and other authors mentioned above strongly favor the former interpretation. First, the convictions data correspond to federal prosecutions, so it is unlikely that there would be substantial cross-sectional variation in the vigor with which prosecutions proceed. Second, the convictions data line up closely with reasonable prior expectations of which states are highly corrupt and which are not so corrupt: Louisiana, Mississippi, and Illinois are among the most corrupt states, and Nebraska, Utah, and New Hampshire are among the not-so-corrupt states. Third, this effect works against our finding a corruption effect on bond characteristics such as yields and ratings, but we find an effect nonetheless. Thus, if anything, our results are conservative estimates of the effects of corruption. Fourth, as discussed above, when we run our tests using the quality-of-state anticorruption laws (that is, the BGA ranking), we obtain identical results, including the same results for the effect of corruption on yields; we therefore conclude that our measure of corruption is not critical for arriving at our most important conclusions. Finally, we note that Fisman and Gatti (2002) test whether these convictions data are determined by cross-state variation in law enforcement and find no significant relation between them.

4.7.3 Reverse causality and codetermination. It seems unlikely that our results arise from a reverse causality story, whereby a state with poor ratings or high bond yields becomes corrupt. That is, higher yields in a state might reflect a greater rate of time preference, making the risk of engaging in corrupt activities for immediate gain relatively attractive. Our view is that this explanation for the relation between corruption and bond characteristics is much less likely than the idea that corruption reflects a priced risk.

Corruption and, say, yields could be codetermined by some unobserved factor (for instance, the moral turpitude of the current legislature or the financial and economic sophistication of the state's leaders and legislators), thereby creating a correlation between the residuals and the convictions measure. One way to rule this possibility out might be to use a state fixed-effects model to capture unobservable factors. However, the within-state variation in corruption is too small to make this empirical strategy suitable. This small within-state variation is not surprising—much of the literature on corruption suggests that corruption is a difficult problem to eradicate.

Our alternative measure of corruption—the quality of anticorruption laws measure mentioned above—provides a compelling rejoinder to the codetermination critique. Because state legislators and voters establish state laws, they provide a measure of corruption potential that is likely to be independent from the unobservable factors mentioned above. Further, laws change very slowly, and surely can be thought of as exogenous to the bond ratings or yields on any given bond issue.

As a further effort to rule out the possibility that a codetermining factor is driving our results, we control for the potential endogeneity of our high corruption dummy variable by jointly estimating corruption and yields with a treatment effects model (untabulated results). The work of Glaeser and Saks (2006) helps us identify an instrument for corruption: the educational development of the state. They show that education is a robust determinant of state-level corruption in the United States. The idea, which they attribute to Lipset (1960), is that a better-educated populace can monitor elected officials and other public figures more effectively. For our purposes, we want a measure of education that is related to corruption in our reduced form equation, but unrelated to the residuals from our structural equation. We use the percentage of the state's population above age 24 that has graduated from high school. This variable is very highly correlated with corruption in our sample, and is arguably unrelated to the residuals from the yields regression after we control for other determinants like per capita income. (We also run our treatment effects model without this instrument, relying on the nonlinearity of the reduced form equation for identification, and find results that are broadly similar). Because we want to examine whether the effect of corruption is different for credit-enhanced and nonenhanced bonds, we run these treatment models on the respective subsamples. As expected, we find results similar to those of the OLS regressions reported in Table 3: instrumented corruption is positively related to bond yields for the nonenhanced sample, with a magnitude very close to that in our OLS regressions. Instrumented corruption is not related to yields in our credit-enhanced sample, just as we find in our OLS tests.

In summary, our main results are robust and it seems unlikely that the various econometric issues discussed above materially affect our conclusions, and if they do, they work against finding our results.

4.7.4 Endogeneity of credit enhancement. Our main results show that corruption is costly, in that it increases municipal bond yields, but that issuers can choose to outsource corruption-related default risk to financial institutions providing credit enhancement. The fact that issuers can choose whether or not to purchase credit enhancement means that credit enhancement is potentially endogenous. Ignoring this could possibly bias the coefficients on our variables of interest, thus overstating our results. Further, if credit enhancement is endogenous, then the interaction between credit enhancement and corruption is also endogenous.

(i) *Endogeneity of credit enhancement: Two-stage least-squares results.* Our first empirical strategy to address the endogeneity of the choice of credit enhancement is to run a 2SLS regression. Our endogenous variable enters our second-stage equation twice: directly as the credit-enhanced indicator variable and again as its interaction with corruption. We instrument separately for each.

We use percentage of bonds the state issued last year that were credit enhanced and the state credit rating, and the interactions of these with corruption as instruments in our first-stage equations. We lose about 11% of our observations (states with no credit rating and the first year of our sample) by requiring these instruments.

It is intuitive that these instruments are related to the credit enhancement choice: states with poor credit ratings are likely to augment the credit quality of the bonds they issue through purchasing credit enhancement, and states that enhanced large proportions of their bonds in the past are likely to continue to do so in the future. State credit rating is unlikely to affect a given bond's yield after controlling for other state characteristics such as corruption, wealth, and state tax rates, and bond characteristics like maturity and issue size. The percentage of bonds the state issued last year with credit enhancement is also unlikely to affect the yields of bonds issued this year.

Although we argue that these instruments are plausibly exogenous, thereby meeting the necessary exclusion restrictions, our method of dealing with endogeneity in the next section does not rely on these instruments.

Table 7 presents the first and second stages of these 2SLS tests. Panel A presents the issue-level tests and panel B presents the regressions of means tests. In all the regressions, we include all the control variables from our OLS tests of the determinants of yields (Table 3), although we omit the control variables from the table to save space. In our first-stage equations, state credit rating and percentage of bonds the state issued last year that were credit enhanced are very strongly related to credit enhancement and weakly related to *credit enhancement* × *corruption*. Conversely, the interaction of state credit rating and percentage of bonds the state issued last year that were credit enhanced with corruption are very strongly related to the *credit enhancement* × *corruption* interaction, but weakly related to credit enhancement alone. This suggests that our instruments are separately identifying the endogenous variables.

In our second-stage regression, corruption now has a much stronger effect on yields than in our OLS tests. The magnitude of the coefficient on corruption now ranges from 19.7 basis points to 38.7 basis points, or roughly three and a half times the magnitude from the OLS tests. This suggests that, in our OLS tests, the magnitude of corruption's effect on yields is being partially obscured by the fact that corrupt states are likely to choose to purchase credit enhancement to mitigate the corruption premium that would be priced into their bond yields. As with our OLS tests, we find that credit enhancement completely eliminates the corruption premium. (We note that, curiously, in the 2SLS tests the coefficient on the *corruption* × *credit enhancement* interaction term is larger in magnitude than the coefficient on the corruption term, and we reject the hypothesis that the sum of the coefficients is zero.)

(ii) *Endogeneity of credit enhancement: Selection model results.* Another empirical strategy to address this problem is to estimate two Heckman (1978) selection models. Though we do not tabulate these results, we describe them here and they are available from the authors upon request. First, we estimate a selection model for the yields of credit-enhanced bonds. Then, we repeat this process, but estimating the selection model for the yields of the non-credit-enhanced bonds. This way we can explicitly model the choice of whether or not an issuer gets credit enhancement and the effect of the choice on yields for each group separately.

We estimate these models relying on the nonlinearity of the first-stage equation for identification (see Maddala 1983). This method substitutes distributional assumptions for the need for valid instruments, such as in our 2SLS approach above. We note that our results are unchanged if we add our instruments from the two-stage least-squares analysis described in the previous section—percentage of bonds the state issued last year that were credit enhanced and the state credit rating—to facilitate the estimation of the selection equation.

The structural equations for yields include all the usual control variables, as well as the inverse mills ratio from the first-stage selection equation. Of course, because we are estimating the structural equations in turn over credit-enhanced and non-credit-enhanced subsamples, we exclude the credit enhancement dummy and the interaction between credit enhancement and corruption. Instead, the direct effect of credit enhancement on yields is captured in the difference in the intercept terms for the two separate structural equations, and the effect of the interaction of corruption and credit enhancement is captured in the difference in the slope coefficient on the corruption variable in the two separate structural equations.

We find results that are qualitatively similar to our 2SLS and OLS tests. When controlling for the selection bias in the choice of whether to obtain credit enhancement, the credit-enhanced bonds have a corruption penalty that is statistically indistinguishable from zero. The nonenhanced bonds have a corruption penalty that is statistically significant and of approximately the same magnitude in the OLS regressions. We conclude that the endogeneity of the credit enhancement decision does not materially impact our results or conclusions.

4.7.5 Alternative tests for underwriter *quid pro quo* during the pay-to-play era. One important alternative specification is to examine whether the pay-to-play premium on negotiated bid bonds is greater in corrupt states. Although our baseline tests in Table 4 include our corruption measure, to address this issue adequately we need to have a variable that is the interaction of corruption, pay-to-play, and the negotiated bid dummy. We augment our specification in Table 4 to regress underwriting gross spreads on our usual variables, plus *Corruption* × *Pay-to-play* × *Negotiated* and lower order interactions. The coefficient on this triple interaction is economically small and statistically indistinguishable from zero; we conclude that the pay-to-play premium on negotiated bid bonds is homogeneous across corrupt and clean states (results not reported).

We then check the robustness of our baseline pay-to-play results in a number of ways. First, although we know from numerous news stories from the time a reasonably precise date of the cessation of pay-to-play, we examine some alternative dates for the shift. We recode our pay-to-play indicator to be, counterfactually, 1993 or 1995. When we do this the magnitude of the pay-to-play result (specifically, the effect of pay-to-play on the negotiated bid deals) diminishes in terms of economic and statistical significance, suggesting that our results are due to the effects of pay-to-play and not to some other structural break surrounding pay-to-play.

Second, we examine whether there is a shift in nonmunicipal debt underwriting gross spreads at about the same time as pay-to-play by looking at gross spreads for *corporate* bond issues. Of course, in corporate underwriting there is no analog of negotiated/competitive bidding or some of the other control variables we use, so we simply regress gross spreads for nonfinancial nonutility nongovernment agency corporate bonds over the same period on some intuitive control variables, such as maturity (logged) and issue size (logged). We include a linear trend variable and a pay-to-play period indicator. As expected, the pay-to-play indicator is statistically indistinguishable from zero.

Third, we examine whether the trend variable is driving the result. We find that it is not. Excluding the time trend increases the magnitude of the pay-to-play-induced shift in gross spreads for negotiated bid deals. This is not surprising, because even a casual inspection of the data suggests that there is a secular decline in spreads during the sample for both competitive and negotiated deals. Thus, not surprisingly, the magnitude of the pay-to-play-induced shift in gross spreads for the competitive bid deals increases as well. However, the resulting estimate of a pay-to-play effect is about three times larger for negotiated bid deals than competitive bid deals.

4.7.6 Additional state-level control variables. One potential concern is that omitted factors, correlated with both corruption and bond yields, are the cause for corruption's impact in our yields regressions (e.g., those in Table 3). Our tests that jointly model corruption and yields (see Section 4.7.3) should allay much of this concern because those tests are designed to minimize the impact of endogeneity and unmodeled omitted factors. Nonetheless, we reestimate our main results adding several control variables intended to capture state-level economic and financial health (results not tabulated). We add to our regression all of the following variables: (a) state size, measured by the natural logarithm of population, (b) state economic vigor, measured by number of business establishments per capita (all industries, from the Bureau of the Census County Business Patterns state-level data), and (c) state financial health, measured by a variant of an interest coverage ratio: (total state revenues minus total state expenditures) divided by interest on general debt. Including the state financial health measure reduces our sample size because we lose 1990 and 1991.

When we include these additional controls, our coefficient estimate of the corruption premium attenuates only slightly (from 0.0668 to 0.0634) and remains statistically significant. Furthermore, our result that credit enhancement eliminates the corruption premium still obtains. In short, it seems that omitted variables are not driving our results, and if omitted variables are driving our results, they must be variables that are not related to our proxies for state size, economic health, and financial health, and that they retain their influence even when we jointly model corruption and yields.

5. Conclusion and Discussion

In this paper we examine how corruption and political connections affect the terms of municipal security offerings. Our paper makes contributions along several dimensions. We show that the political integrity of municipal bond issuers is priced and that rating agencies implicitly treat corruption as a component of a bond's overall default risk. This intracountry issue-level result is consistent with findings that country-level institutional quality affects sovereign debt ratings and yields. Country-level studies, however, are silent on how institutional quality affects the choice of financial intermediaries in financial transactions. Our findings should be of particular interest to financial economists because we show a new and unique role that financial institutions play in financial transactions: issuers can outsource corruption-induced default risk to institutions that provide credit enhancements.

Because these institutions insure or otherwise enhance many bond issues, mostly from municipal issuers (see Nanda and Singh 2004), they are specialists at evaluating default risk that can arise from political malfeasance and corruption-related activities. Thus, they have a comparative advantage at information production over the investors in the bonds (who may not have the economies of scale needed to make it worthwhile to develop similar evaluation technologies). Accordingly, credit-enhancing institutions act as delegated screeners to establish a price for the political risk inherent in the bonds. Our findings should also be of interest to development economists because we identify a way to mitigate the damaging effects of corruption even if/when corruption itself cannot be completely eliminated. Further, the results should be of immediate and direct relevance to government entities wishing to raise external capital.

Our research design also allows us to examine the interaction of underwriters with issuers and markets. Though pay-to-play created the potential for favoritism, we believe we are the first to document empirically a channel for the benefits that underwriters received in exchange for campaign contributions. During the pay-to-play era, underwriters appear to have received *quid pro quo* for political campaign contributions in the form of higher underwriting fees for negotiated bid offerings.

On balance, our findings provide insight as to how financial market participants might deal with even severe corruption: use certification and/or guarantees by outside parties to mitigate the problems that corruption creates.

Appendix

This appendix defines all our variables.

Variable name	Description
Corruption Measures	
Convictions per million population	The number of federal corruption convictions per capita times one million
Convictions top quartile	One of our main measures of corruption. It is an indicator variable for state-years in the top quartile of federal convictions per capita; the variable takes a value of 1 if the state-year is in the top quartile of corruption convictions
BGA rank	The state ranking of the quality of state anticorruption laws that is produced by the Better Government Association, a civic watchdog group; higher numbers correspond to lower quality of state anticorruption laws
BGA $\geq$*40*	One of our main measures of corruption. It is an indicator variable for state ranking of the quality of state anticorruption laws greater than or equal to 40; a value of 1 indicates a high (that is, poor) ranking from BGA, whereas a value of 0 indicates a good ranking
Bond Characteristics	
Yield	The bond's yield to maturity at issuance
Gross spread	Total underwriting fee measured as a percentage of issue size
Size	Bond size, measured in millions of dollars of proceeds. We use the natural logarithm of this variable in our regressions
Time to maturity	The time to maturity of the bond, measured in years. We use the natural logarithm of this variable in our regressions
Credit enhancement	An indicator variable for the bond having credit enhancements such as bond insurance or letter of credit backing. The variable takes a value of 1 for bonds with any credit enhancement and 0 otherwise
GO bond	An indicator variable that takes a value of 1 for bonds that are general obligation bonds for the state and 0 otherwise
Negotiated bid	An indicator variable that takes a value of 1 for bonds for which the underwriter is engaged through a negotiated offer and 0 otherwise
Minority	An indicator variable that takes a value of 1 for bonds for which the lead underwriter is owned by minorities
Underwriter market share	The underwriter market share measured as a percentage of total municipal bond value underwritten by a particular underwriter during the year. We use the natural logarithm of this variable in our regressions
Bond rating	A numerical categorization of the bond's credit rating assigned by a rating agency. We use S&P ratings where they are available, and Moody's otherwise. The lowest quality bonds are assigned the value 0, and we add 1 for each increment in credit rating for a maximum value of 21. When the bond is not rated and we want to include the bond rating variable in a regression, we code this variable with a value of -1 and include a dummy variable to capture the fact that the bond is not rated (defined next)
No rating	An indicator for the bond not having any credit rating from S&P or Moody's
Matching treasury	The nominal rate on a treasury security of similar maturity
State Characteristics	
State rating	Moody's credit rating for the state's general obligation bonds, where we assign the value of 0 for the lowest quality bonds and add 1 for each increment in credit rating, with the maximum possible value equal to 21
State % bonds with credit enhancement last year	The percentage of all bonds issued by state i in year $t-1$ that were credit enhanced
GSP per capita	The gross state product divided by the state's population
Tax	The highest marginal personal state income tax rate for the issuing state

Variable name	Description
Other Variables	
Year dummies	Indicator variables that take a value of 1 for a particular year and 0 otherwise
Region dummies	Indicator variables that take a value of 1 for a particular region of the country and 0 otherwise. The region dummies divide the country into four parts as classified by the U.S. Census Bureau: West, Midwest, South, and Northeast
Pay-to-play	An indicator that identifies the pay-to-play era. The variable takes a value of 1 if the bond was issued before April 1994, 0 otherwise. When we use the pay-to-play indicator with issue level tests, we omit observations in the months immediately surrounding April 1994 (i.e., 31 March through 1 July 1994)
Time trend	A linear trend. It takes a value of 0 for the first year in the sample (1990), a value of 1 for the next year, and so on

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