

Corruption, Political Connections, and Municipal Finance

Alexander W. Butler
Larry Fauver
Sandra Mortal

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Butler is at University of Texas at Dallas. Fauver is at University of Tennessee. Mortal is at University of Missouri. Send correspondence to the first author at: Department of Finance, School of Management, SM 31, The University of Texas at Dallas, Richardson, TX 75083. Email: butler@utdallas.edu.

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Abstract

We exploit unique features of the U.S. municipal bond underwriting market to assess how political integrity affects primary financial market outcomes. We show that state corruption and political connections have strong effects on several aspects of municipal bond sales and underwriting. Specifically, we find that higher state corruption is associated with greater credit risk, higher bond yields, greater use of external credit enhancement, and use of lower quality underwriters. States that are more corrupt can eliminate the corruption yield penalty by purchasing credit enhancements, effectively selling integrity-related default risk to an independent financial intermediary. Underwriting fees do not vary with cross-state corruption, but were significantly higher during an era when underwriters routinely made political campaign contributions to win underwriting business. Furthermore, 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 state corruption and political connections on economic and financial outcomes.

Key words: Political integrity, Corruption, Pay to play, Municipal finance

JEL Categories: 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 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 intra-country empirical design, we exploit the distinctive features of the U.S. municipal bond underwriting market to identify the effects of political integrity on these primary financial market transactions. We use the temporal and cross-sectional variation in state corruption in the U.S. to gain unique insights into this issue.

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, Glaeser and Saks (2006), Fisman and Love (2002), Fredrickson, et al. (2003), Depken and LaFountain (2006)).² This measure views states with low levels of per capita corruption convictions as having high political integrity, and vice versa. Temporal variation comes from abrupt changes in the extent to which municipal bond underwriting involves "pay to play" (discussed in more detail below), 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

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

² We also use a proxy based on the stringency of anti-corruption laws in the state. We discuss this in more detail below.

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 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 worse bond rating than that of a less corrupt state.

³ How might state corruption affect the credit risk and in turn the costs of issuing municipal debt? Two recent *Wall Street Journal* articles (Richardson (2005) and Whitehouse (2005)) indicate various ways in which 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." 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. 04/21/1995). 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'être* of the bond issues. The bonds ultimately defaulted after the junk bond market crashed and the insurance company was placed into conservatorship.

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*. Overall, the corruption penalty on yields is a statistically significant 9.3 basis points. The corruption-related default risk is partially reflected in the bond ratings assigned by rating agencies. When we control for the bond's credit rating, corruption's effect on bond yields decreases by about one-fourth to 7.0 basis points. For comparison, bond yields increase by 3.3 basis points per rating notch, so the corruption premium is roughly the equivalent of a bond issue being rated more than 2.1 notches worse (e.g., from A- to below BBB), *ceteris paribus*. This effect continues to hold with underwriter fixed effects—with the same underwriter, a bond issued by a corrupt state has a yield that is a statistically significant 5.1 basis points higher on average, other things equal.

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. (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.) However, revealed preference suggests that the costs of corruption exceed the costs of credit enhancements: it is precisely the most corrupt states that are most likely to purchase credit enhancements. We view this as an important result, because it documents a channel through which corruption's *effects* (as opposed to the level of corruption itself) can be attenuated and

suggests an important role for financial institutions (i.e., institutions providing credit enhancements) in alleviating the economic damage that corruption can cause.

Third, we ask whether political integrity affects the pricing of underwriting services (specifically, underwriting gross spreads). 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. 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—14.0 basis points, or roughly one-seventh of the mean gross spread. In contrast, competitive deals, which offer no room for favoritism, have fees that are only 0.6 basis points higher (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, et al. (2006), Butler and Fauver (2006)), but not what market participants might do about it. Further, while Khwaja and Mian (2005) show how government-owned institutions (government-owned banks) can be the *facilitators* of corrupt practices, we show how a private financial institution (i.e., a credit-enhancing institution) can offer a *solution* to corruption's effects. 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.

Note that our findings can be interpreted as the "tip of the iceberg." Compared to other countries, the United States is relatively free from corruption. It therefore seems reasonable to think that the interstate variation in corruption we find is very small compared to the intercountry variation in corruption. For example, the difference in corruption between Louisiana and Nebraska is likely to be tiny compared to the difference in corruption between Equatorial Guinea and Denmark. Thus, it seems reasonable to think that the effects that we document would be even more dramatic in a cross-country comparison.

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 a "wealthy state" effect or other state-level macroeconomic effects because we find that its effects continue to hold when we control for state wealth across time. We address a variety of selection, endogeneity, data, robustness, variable construction, and interpretation concerns in Section IV, subsection G.

The remainder of the paper is organized as follows. Section I provides background information about the primary market for municipal securities, security underwriting, and the pay-to-play era. In Section II we discuss the data and methods that we use. Section III discusses the characteristics of the data. Section IV presents our main multivariate results. Section V concludes and provides a discussion for the generality and applicability of our results.

I. 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 1 to 30 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 general obligation 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 do GO bonds 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)). Though recent regulatory scrutiny appears to have effectively eradicated corruption in the industry, 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, et al. (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. The initial draft of the self-regulatory plan was written in 1993 under the direction of Frank Zarb, then Chairman of Primerica, the parent company of Smith Barney Shearson, a major municipal bond underwriting firm at the time (Fuerbringer (1993)). In April 1994, the SEC established a rule that investment houses making political contributions could not sell bonds from that city/state for 2 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), who alleged that the SEC's stifling of pay-to-play was a violation of first and tenth amendment rights (Wayne (1994)), but the suit was not successful (Gasparino (1998)). The SEC's pressure had its intended effect and pay-to-play is no longer prevalent in municipal underwriting.

II. Data, methods, and research design

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 Public Integrity Section, and similar ex post measures of corruption have been widely used by Glaeser and Saks (2006), Fredricksson, List, and Millimet (2003), Fisman and Gatti (2002), 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.

As an additional measure of corruption, we use a measure of the quality of state anti-corruption 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 worse quality of laws. This is so that all our measures of corruption are positively related, i.e. higher values of measure, higher corruption. Unlike the convictions measures for which we have a panel, we have only cross-sectional variation in the BGA index.

We obtain data on municipal bond issues from the Securities Data Company (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 percent 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., general obligation 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 the 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 general obligation 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

Meggison and Weiss (1991).⁴ 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. Note that the number of bond issues varies widely across states. Treating each bond issue equally would effectively overweight the heavy-issuing states and influence the regression coefficients, especially on state-level variables such as corruption. Our interest lies in how state corruption affects bond issuance, so this is an important consideration. To deal with the wide variation in the number of issues across states, we use weighted regressions where each bond issue is weighted by the inverse of the number of issues from that state to ensure each state is given identical importance. A number of cross-country studies also use weighted regressions to address similar data issues (see, e.g., Love (2003), Khurana et. al. (2006), and Yeung et. al. (2006)). For robustness, we also run regressions without this weighting. We find qualitatively similar results (except for our regressions of credit enhancement determinants, where the coefficient on the corruption measure becomes statistically insignificant).

< Insert Table 1 Here >

III. Descriptive statistics and univariate results

⁴ We note that a common alternative, “tombstone rankings,” based on Carter and Manaster (1990) and Carter, Dark, and Singh (1998) are not appropriate here because they are based on *equity* underwriting.

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. Approximately 25% of the (state-weighted) bond issues are issued by states in the top quartile of federal convictions (our *Corruption* measure) per capita. 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 yield to maturity at the time of the bond issue is 4.39% (the yield ranges from 4.04% for New Jersey to 4.85% for West Virginia). The average gross spread is 1.08% and nearly one-third of the bond issues have credit enhancements. The 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 12 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.

In Table 1, Panel B we further examine the cross-sectional statistics of our municipal bond sample for all bonds. The statistics show that nearly one-half of the bond offerings are not rated and that 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 1 to 30 years.

IV. 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 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 other measures: convictions per population, and an indicator for the state being in the bottom quintile of quality of anti-corruption laws (based on the BGA rank). We also report results for unweighted regressions.

A. Determinants of bond ratings

Table 2 examines the determinants of bond ratings. Because bond ratings are ordinal rather than continuous, we use an ordered logit model (we note that using a simple OLS specification gives similar results). Our interest is in how corruption in the state 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.

<Insert Table 2 Here>

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. 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 non-corrupt state. This result also holds for unweighted regressions, but neither convictions per population or BGA rank are statistically significant. OLS results (not tabulated) indicate that the magnitude of this effect is about half a rating notch. All else equal, a general obligation bond and a larger bond offering result in a higher bond rating. Other independent variables that affect bond ratings are the negotiated bid indicator (negotiated deals have better ratings, *ceteris paribus*), and gross state product per capita (wealthy states have better bond ratings, *ceteris paribus*).

B. Determinants of bond yields

Table 3 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. In the second model specification, we also include the bond rating and an indicator for a non-rated bond.

From these yield regressions, we conclude that corruption increases bond yields. Bonds issued by a corrupt state have yields 9.35 basis points higher than bonds issued by

non-corrupt states, holding other factors constant. This result also holds with underwriter fixed effects (the third regression model we present in the table), which means that when the same underwriter brings a bond to market for a corrupt issuer, the yield is 5.14 basis points higher than if they had brought to market a non-corrupt issuer's bond with the same characteristics.

<Insert Table 3 Here>

The impact of corruption on yields is attenuated when we include as control variables the bond's rating and the non-rated indicator variable. The coefficient on the corruption indicator becomes 0.0704, or 7.04 basis points, which is a 25% decrease in magnitude from the model that does not control for ratings. 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.

We find similar results for our alternative measures of corruption and when we use unweighted regressions. The corruption penalty on bond yields is 2.1 basis points per corruption conviction per million in population. A one standard deviation increase in convictions per population corresponds to a 4.4 basis point increase. A state with the worst quintile BGA rank has a corruption penalty of 11.5 basis points.

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 of 48% or higher. This implies that third-party certification can alleviate the risks associated with corruption. We document this result graphically in Figure 1, where we present a partial regression plot based on the coefficients of the first model in Table 3.

<Insert Figure 1 Here>

Other control variables 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, general obligation 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 non-rated 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 effect on bond yields.

C. 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 as well as logged issue size, state tax rate, logged maturity, logged underwriter market share, Gross State Product per capita, a variable that interacts corruption and credit enhancement, and year and region dummies. For the last three models in Table 4 we exclude the year dummies and include a time trend, a pay-to-play indicator (defined below), and an interaction term for pay-to-play and negotiated bid.

We find no evidence that corruption has any systematic impact on investment banking fees. This non-result is very robust, and holds for each of our corruption measures. Therefore, for parsimony we report only the results based on our high corruption indicator. We observe that minority underwritten bonds, a larger offer size, and underwriter quality reduce gross spreads. Negotiated bids and increased time to maturity increase gross spreads. Negotiated bids are arguably more complicated, which could cause underwriters to charge more for their services.

<Insert Table 4 Here>

D. Underwriter fees and the effect of pay-to-play

In the last three columns of Table 4 we extend our analysis by adding the effect of the pay-to-play era on gross spreads. 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. 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, 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 any 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 0.6 basis points, which reflects the average change for competitive bid deals. This coefficient is

not statistically significant (p -value of 84%). However, the coefficient on the interaction between pay-to-play and negotiated bids is a statistically significant 14.0 basis points. (We note that we find essentially identical results for unweighted regressions.) This means that, other things equal, negotiated bid deals had underwriter gross spreads of 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 present this result graphically in Figure 2, which is a partial regression plot of the second model in Table 4.

<Insert Figure 2 Here>

We find the same results when we control for underwriter fixed effects. The coefficient on the interaction term attenuates slightly to 11.2 basis points. The interpretation here is that holding bond and state characteristics constant, the same underwriter, working on a negotiated bid deal charged significantly more during pay-to-play than if they had underwritten the same bond after pay-to-play. We discuss some robustness tests in section IV.G below.

E. 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 credit enhancement choice. We run logistic regressions with credit enhancement as the dependent variable to determine what leads issuers to purchase credit enhancements. In Table 5 we report the regressions, where the dependent variable takes a value of 1 if the bond has any type of credit enhancement, and 0 otherwise (we note that when we do this analysis for particular types of enhancements, such as bond insurance, letter of credit backing, etc., we find very

similar results). We find that corrupt states are more likely to use credit enhancements based on each of our three measures of corruption.⁵

<Insert Table 5 Here>

We interpret these results as follows. States that are corrupt pay a premium on their bonds due to the increased risk 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. That is, the cost of credit enhancements must be less than the benefit the corrupt state issuer gains from a reduced yield, on average. This net benefit 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 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.

F. Determinants of underwriter choice

In Table 6 we examine the determinants of issuers' choice of underwriter reputation. Following previous literature on underwriter reputation, we use the underwriter's market share in a given year as our proxy for reputation. We estimate this variable in two ways for each underwriter in our sample. First, and more traditionally, we use the underwriter's market share in the market we are studying—municipal bond underwriting. We speculate that underwriters might gain market share through pay-to-

⁵ Using unweighted regressions, corruption is not statistically significant. This is the only case where we get qualitatively different results using unweighted regressions.

play activities, which could increase their market share, but this increase in market share would not necessarily indicate a higher reputation. Our second measure of underwriter reputation is based on annual market share in the *corporate* debt underwriting market. We suspect that an underwriter with a high reputation in one of these markets would be likely to have a high reputation in the other as well. However, pay-to-play activities would not affect market share in corporate debt underwriting. Although this may be a noisy measure of reputation in the municipal underwriting market, we note that because we use this as our dependent variable any measurement error goes into the residuals.

<Insert Table 6 Here>

In the first two regressions and the last three regressions we use as our dependent variable underwriter quality in the municipal bond market; the third regression uses underwriter quality in the corporate bond market. In all of the regressions we observe that more corrupt states use lower reputation underwriters on average, holding other factors constant. The lone exception is the model that uses convictions per population as a corruption measure; in this model corruption is not statistically significant. The first regression model presents our baseline results, namely, that corrupt states use less reputable underwriters, minority-owned investment banks have lower market share, and larger and longer maturity issues use better underwriters.

The second model specification replaces the year dummies that we use in the first model with a pay-to-play indicator. We 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.)

In the third model, we compute the *corporate* bond underwriting market share for the underwriter bringing the municipal bond to market. All the results here are

qualitatively similar to those in the first model, except that maturity is no longer statistically significant. The fourth model uses convictions per population as the corruption measure (not significant). The fifth model uses the BGA rank measure of corruption and we find similar results to our baseline specification. The last model uses unweighted regressions and we find similar results to our baseline specification.

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 securities for 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.

G. 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.

G.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 relationship between corruption and issuance characteristics such as yields and ratings. Thus, our results are conservative estimates of the impact of corruption. However, this discussion is largely moot because corrupt states issue

approximately the same number of bonds, and bonds of about the same issue size, as non-corrupt states.

G.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 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 anti-corruption laws (that is, the Better Government Association ranking), we obtain 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.

G.3. Reverse causality and co-determination

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 co-determined 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 anti-corruption laws measure mentioned above—provides a compelling rejoinder to the co-determination critique. Because state laws are established by state legislators and voters, 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.

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

G.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.

Our empirical strategy to address this problem is to estimate two Heckman (1978) selection models.⁶ 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.

To facilitate the estimation of the selection equation, we add two regressor variables that help predict credit enhancement, but which are arguably exogenous to the residuals in the structural equation for yields: percentage of bonds the state issued last year that were credit enhanced and the state credit rating. We lose some observations because we lose the first year of data, 1990, because we use it to estimate the previous-year credit enhanced percentage for 1991. We also lose observations for five states that have no state credit rating. These appear to be valid instruments because they are very strong predictors of the credit enhancement choice. We note that our results are unchanged if we omit these instruments, and rely solely on the non-linearity of the first stage equation for identification (see Maddala (1983)).

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

⁶ A simple two stage least squares approach does not work here because of the extreme multicollinearity created by instrumenting for both credit enhancement and the interaction between credit enhancement and corruption. (See Wooldridge (2003), p. 502 for a straightforward exposition of how such problems can arise.) Moreover, treatment effects models are inappropriate here because we are interested in not just one potentially endogenous dummy variable, but also its interaction with corruption.

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 and quantitatively similar to our 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 non-enhanced 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.

G.5. Alternative tests for underwriter quid pro quo during the pay-to-play era

We check the robustness of our 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.

Second, we examine whether there is a shift in non-municipal 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 non-financial non-utility non-government 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.

V. 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 intra-country 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.

Can we infer from these intra-country findings the presence of country-level effects of corruption? Yes and no. Compared to many countries, the U.S. as a whole has very low corruption (see, for example, Mauro (1995) and Butler and Fauver (2006)). Indeed, some foreign countries have such severe corruption or poor systems of law and property rights that the financial intermediaries needed to attenuate corruption's effects may be absent or unwilling to participate in those markets. One should therefore be cautious about extrapolating from the results above to questions about country-level corruption. On the other hand, because corruption is so much higher in other countries, the magnitude of the benefits that could be conveyed by a financial intermediary that are

identified here might be very large. On balance, our findings do give some insight as to how financial market participants might deal with even severe corruption: use certification and/or guarantees by outside parties. Extending the analysis of this paper to the cross-country setting is left for future research.

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Table 1**Summary statistics**

Summary statistics for pooled bond characteristics by state and overall sample. The sample comprises 127,976 observations. In Panel A we report for each state, the number of bond issues (column heading - Count) and means of bond characteristics. 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. The variables are as follows. “Convict. top quart.” is an indicator variable for state-years in the top quartile of federal convictions; “Convict. per mill. pop.” is the number of federal convictions per million population; “BGA rank” is the state ranking of the quality of state anti-corruption laws that is produced by the Better Government Association, a civic watchdog group, where a higher number corresponds to worse quality of state anti-corruption laws. “State rating” is the 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; “Bond rating” is the bond’s credit rating assigned by S&P when available and Moody’s otherwise; “No rating” is an indicator for the bond not having any credit rating; “Yield” is the bond’s yield to maturity at issuance; “Gross spd” is gross spread and measured as a percentage of issue size; “Cred. enh.” is an indicator variable for the bond having credit enhancements; “Uw. mkt. share” is the underwriter market share measured as a percent of total municipal bond value underwritten by a particular underwriter during the year; “Size” is bond size; “Time to maturity” is measured in years; “GO bond” is an indicator variable for the bond being general obligation; “Neg. bid” is an indicator variable for the underwriter having been engaged through a negotiated offer; and “Minority” is an indicator for the bond having been issued by a lead-underwriter that is owned by minorities.

Panel A

State	Count	Convict. top quart. (%)	Convict. Per mill. pop.	BGA rank	State rating	Bond rating	No rating (%)	Yield (%)	Gross spd. (%)	Cred. enh. (%)	Uw. mkt. share (%)	Size (\$ mils.)	Time to maturity (yrs.)	GO bond (%)	Neg bid (%)	Minority (%)
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
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

Table 1 (cont.)

State	Count	Convict. top quart. (%)	Convict. Per mill. pop.	BGA rank	State rating	Bond rating	No rating (%)	Yield (%)	Gross spd. (%)	Cred. enh. (%)	Uw. mkt. share (%)	Size (\$ mils.)	Time to maturity (yrs.)	GO bond (%)	Neg bid (%)	Minority (%)
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

State	Count	Convict. top quart. (%)	Convict. Per mill. pop.	BGA rank	State rating	Bond rating	No rating (%)	Yield (%)	Gross spd. (%)	Cred. enh. (%)	Uw. mkt. share (%)	Size (\$ mils.)	Time to maturity (yrs.)	GO bond (%)	Neg bid (%)	Minority (%)
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	4671.52	30.00	100.00	100.00	100.00

Table 2**Bond rating determinants**

Ordered logit regressions of bond ratings for a sample of non-credit enhanced issues. For each 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 (or from a 0 to 1 change if an indicator variable), while keeping all other variables constant at their means. 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. The main variable of interest is corruption, which we measure in three different ways, and identify in column headings. Specifically, we measure corruption with an indicator variable for state-years in the top quartile of federal convictions; number of convictions per million population; and an indicator variable for state ranking of the quality of state anti-corruption laws greater than or equal to 40. This ranking is produced by the Better Government Association and a higher number corresponds to worse quality of state anti-corruption laws. The remaining independent variables are: “Minority indicator”, which identifies deals in which the lead-underwriter is a minority-owned firm; “GO bond indicator”, which identifies general obligation bonds; “Negotiated bid indicator”, which identifies deals where the underwriter is engaged through negotiated bidding; “Ln(Size)”, the natural logarithm of bond size; “Ln(Maturity)”, the natural logarithm of bond maturity; “Ln(Udw. mkt. share)”, the natural logarithm of underwriter market share measured as the percentage of total municipal bond value underwritten by that particular underwriter during the year; and “GSP per capita”, the Gross State Product as a proportion of the state’s population. The first three regressions are inversely weighted by the number of state-year bonds, while the last regression is a non-weighted version of the first regression model. All regressions include year and region dummies (not tabulated). Region dummies divide the country into four parts as classified by the U.S. Census Bureau: West, Midwest, South, and Northeast. Robust *p*-values (in parentheses) take into consideration standard errors that are adjusted for within-state clustering.

	Convictions top quartile ind.		Convictions per mill. population		BGA rank >= 40 ind..		Non-weighted	
	Coeff.	Elast.	Coeff.	Elast.	Coeff.	Elast.	Coeff.	Elast.
Corruption measure	-0.3947** (0.035)	-0.0428	-0.0517 (0.163)	-0.0131	-0.2864 (0.412)	-0.0314	-0.3540** (0.034)	-0.0299
Minority indicator	-0.3930 (0.123)	-0.0402	-0.3806 (0.135)	-0.0394	-0.3965 (0.111)	-0.0404	-0.1508 (0.272)	-0.0130
GO bond indicator	1.3508*** (0.000)	0.1284	1.3606*** (0.000)	0.1301	1.3722*** (0.000)	0.1299	1.0932*** (0.000)	0.0831
Negotiated bid indicator	0.2400* (0.058)	0.0290	0.2486* (0.051)	0.0303	0.2466* (0.050)	0.0298	0.0143 (0.927)	0.0013
Ln(Size)	0.5641*** (0.000)	0.0960	0.5637*** (0.000)	0.0966	0.5676*** (0.000)	0.0965	0.5189*** (0.000)	0.0689
Ln(Maturity)	0.0639 (0.751)	0.0037	0.0619 (0.761)	0.0036	0.0487 (0.807)	0.0028	0.0688 (0.545)	0.0031
Ln(Udw. mkt. share)	1.3672 (0.370)	0.0048	1.4391 (0.352)	0.0051	1.3974 (0.354)	0.0049	2.9434*** (0.003)	0.0080
GSP per capita	0.0552* (0.078)	0.0440	0.0558* (0.068)	0.0448	0.0597* (0.069)	0.0476	0.0477* (0.081)	0.0296
Year dummies	Yes		Yes		Yes		Yes	
Region dummies	Yes		Yes		Yes		Yes	
Pseudo-R ²	0.0910		0.0902		0.0903		0.0790	
No. of observations	30,259		30,259		30,259		30,259	

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3**Yield determinants**

Regressions of yields to maturity. We present various regression specifications including underwriter fixed effects (third regression). The main variable of interest is corruption, which we measure in three different ways, and identify in column headings. Specifically, we measure corruption with an indicator variable for state-years in the top quartile of federal convictions; number of convictions per million population; and an indicator variable for state ranking of the quality of state anti-corruption laws greater than or equal to 40. This ranking is produced by the Better Government Association and a higher number corresponds to worse quality of state anti-corruption laws. The remaining independent variables are: “Credit enhancement indicator”, which identifies deals with credit enhancement; an interaction of the corruption measure with Credit enhancement; “Minority indicator”, which identifies deals in which the lead-underwriter is a minority firm; “GO bond indicator”, which identifies general obligation bonds; “Negotiated bid indicator”, which identifies deals where the underwriter is engaged through negotiated bidding; “Ln(Size)”, the natural logarithm of bond size; “Ln(Maturity)”, the natural logarithm of bond maturity; “Ln(Underwriter mkt. share)”, the natural logarithm of underwriter market share measured as the percentage of total municipal bond value underwritten by that particular underwriter during the year; “Bond rating” is the bond’s credit rating assigned by S&P when available and Moody’s otherwise; 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, and when the bond is not rated we code this variable with a value of -1; “No rating” is an indicator for the bond not having any credit rating; “Matching treasury” is the nominal rate on a treasury security of similar maturity; “Tax” is the highest marginal personal state income tax rate for the issuing state; and “GSP per capita” is the Gross State Product as a proportion of the state’s population. All regressions are inversely weighted by the number of state-year bonds, except the last, which is the non-weighted version of the first regression. All regressions include year and region dummies (not tabulated). Region dummies divide the country into four parts as classified by the U.S. Census Bureau: West, Midwest, South, and Northeast. Robust *p*-values (in parentheses) take into consideration standard errors that are adjusted for within-state clustering.

	Convictions top quart. ind.	Convictions top quart. ind.	Convictions top quart. ind.	Convictions per mill. pop.	BGA rank >= 40 ind.	Non- weighted
Constant	2.2831*** (0.000)	2.3912*** (0.000)	2.4095*** (0.000)	2.2342*** (0.000)	2.2296*** (0.000)	2.3071*** (0.000)
Corruption measure	0.0935*** (0.004)	0.0704** (0.027)	0.0514** (0.041)	0.0205*** (0.003)	0.1146*** (0.005)	0.0668* (0.078)
Corrupt X cred. enh.	-0.0785* (0.078)	-0.0583 (0.230)	-0.0493 (0.150)	-0.0192 (0.132)	-0.0546 (0.587)	-0.0613 (0.168)
Credit enhancement indicator	-0.2638*** (0.000)	-0.0550* (0.090)	-0.2334*** (0.000)	-0.2268*** (0.000)	-0.2733*** (0.000)	-0.2666*** (0.000)
Minority indicator	-0.0255 (0.504)	0.0041 (0.901)	-0.2634 (0.239)	-0.0267 (0.480)	-0.0247 (0.510)	-0.0244 (0.498)
GO bond indicator	-0.2093** (0.000)	-0.1478*** (0.000)	-0.1681** (0.000)	-0.2093*** (0.000)	-0.2121** (0.000)	-0.2149*** (0.000)
Negotiated bid indicator	0.1075*** (0.000)	0.0680*** (0.001)	0.0753*** (0.003)	0.1082*** (0.000)	0.1047*** (0.000)	0.1354*** (0.000)
Ln(Size)	-0.0462*** (0.000)	-0.0069 (0.414)	-0.0376*** (0.000)	-0.0470*** (0.000)	-0.0453*** (0.000)	-0.0491*** (0.000)
Ln(Maturity)	0.2328*** (0.000)	0.3031*** (0.000)	0.2210*** (0.000)	0.2335*** (0.000)	0.2283*** (0.000)	0.2230*** (0.000)
Ln(Underwriter mkt. share)	0.0759 (0.755)	0.2778 (0.247)	0.6032 (0.237)	0.0758 (0.758)	0.0947 (0.690)	0.3526 (0.208)
Bond rating		-0.0333*** (0.001)				
No rating indicator		-0.2557* (0.080)				
Matching treasury	0.4328*** (0.000)	0.4352*** (0.000)	0.4325*** (0.000)	0.4331*** (0.000)	0.4328*** (0.000)	0.4347*** (0.000)
Tax	-0.0048 (0.297)	-0.0099** (0.046)	-0.0106*** (0.009)	-0.0044 (0.322)	-0.0024 (0.562)	-0.0073 (0.118)
GSP per capita	0.0142*** (0.004)	0.0155*** (0.001)	0.0079** (0.045)	0.0143*** (0.003)	0.0169*** (0.002)	0.0093** (0.014)
Underwriter FE	No	No	Yes	No	No	No
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Region dummies	Yes	Yes	Yes	Yes	Yes	Yes
F-test: Corrupt + (corrupt x cred. enh.) = 0	0.15 (0.7044)	0.10 (0.7557)	0.00 (0.9500)	0.01 (0.9122)	0.54 (0.4676)	0.02 (0.8805)
R-squared	0.762	0.774	0.786	0.763	0.763	0.748
No. of observations	88,588	88,588	88,588	88,588	88,588	88,588

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 4**Gross spread determinants**

Regressions of underwriter gross spreads. The independent variables are: “Convictions top quartile indicator”, which identifies state-years in the top quartile of federal convictions; “Credit enhancement indicator”, which identifies deals with credit enhancement; an interaction of Corrupt indicator and Credit enhancement; “Minority indicator”, which identifies deals in which the lead-underwriter is a minority-owned firm; “GO bond indicator”, which identifies general obligation bonds; “Negotiated bid indicator”, which identifies deals where the underwriter is engaged through negotiated bidding; “Ln(Size)”, the natural logarithm of bond size; “Ln(Maturity)”, the natural logarithm of bond maturity; “Ln(Underwriter mkt. share)”, the natural logarithm of underwriter market share measured as the percentage of total municipal bond value underwritten by that particular underwriter during the year; “Bond rating” is the bond’s credit rating assigned by S&P when available and Moody’s otherwise; 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, and when the bond is not rated we code this variable with a value of -1 “No rating” is an indicator for the bond not having any credit rating; “Tax”, the highest marginal personal state income tax rate for the issuing state; and “GSP per capita”, the Gross State Product as a proportion of the state’s population. Regressions either include year dummies (not tabulated) or a “Pay-to-play indicator” variable, which identifies the pay-to-play era (i.e., the bond was issued before April 1994), and interaction of the pay-to-play indicator with the negotiated bid indicator, and a “Time trend”, which counts for the year of the issue, where first sample year takes the value of 0. When using the pay-to-play indicator we omit observations in the months immediately surrounding April 1994 (i.e., March 31st through July 1st 1994). All regressions are inversely weighted by the number of state-year bonds, except the last, which is the non-weighted version of the first regression. All regressions include region dummies (not tabulated). Region dummies divide the country into four parts as classified by the U.S. Census Bureau: West, Midwest, South, and Northeast. Robust *p*-values (in parentheses) take into consideration standard errors that are adjusted for within-state clustering.

	Year dummies	Pay-to-play	Underwriter Fx. Effects	Non-weighted
Constant	0.0825 (0.751)	-0.0647 (0.785)	0.5653** (0.025)	0.2022 (0.268)
Convictions top quartile indicator	-0.0416 (0.282)	-0.0493 (0.205)	-0.0034 (0.819)	-0.0247 (0.449)
Corrupt X cred. enh.	-0.0113 (0.756)	0.0034 (0.926)	-0.0127 (0.583)	-0.0070 (0.836)
Credit enhancement	0.0352 (0.570)	0.0313 (0.611)	0.0568 (0.223)	-0.0216 (0.586)
Minority indicator	-0.1869** (0.000)	-0.1891*** (0.000)	0.0449 (0.759)	-0.1178** (0.012)
GO bond indicator	0.0068 (0.874)	0.0064 (0.879)	-0.0060 (0.874)	-0.0283 (0.352)
Neg. bid indicator	0.2699** (0.000)	0.2096*** (0.000)	0.1188*** (0.008)	0.1799*** (0.000)
Ln(Size)	-0.1420*** (0.000)	-0.1426*** (0.000)	-0.1257*** (0.000)	-0.1487*** (0.000)
Ln(Maturity)	0.4221*** (0.000)	0.4201*** (0.000)	0.3755*** (0.000)	0.3875*** (0.000)
Ln(Underwriter mkt. share)	-1.6216*** (0.002)	-1.6108*** (0.002)	0.1227 (0.808)	-1.4223*** (0.000)
Bond rating	-0.0081 (0.586)	-0.0094 (0.513)	-0.0188 (0.178)	0.0010 (0.897)
No rating indicator	0.1651 (0.404)	0.1404 (0.455)	-0.0785 (0.676)	0.2882** (0.017)
Tax	0.0094 (0.306)	0.0099 (0.274)	-0.0010 (0.865)	0.0030 (0.700)
GSP per capita	0.0060 (0.223)	0.0063 (0.189)	0.0033 (0.229)	0.0003 (0.952)
Pay-to-play indicator		0.0060 (0.839)	0.0141 (0.645)	0.0071 (0.749)
Pay-to-play indicator X neg. bid indicator		0.1398*** (0.001)	0.1116*** (0.000)	0.1384*** (0.000)
Time trend		-0.0226** (0.010)	-0.0238*** (0.000)	-0.0149** (0.035)
Underwriter fixed effects	No	No	Yes	No
Year dummies	Yes	No	No	No
Region dummies	Yes	Yes	Yes	Yes
R-squared	0.361	0.364	0.459	0.360
No. of observations	40,514	39,764	39,764	39,764

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5
Credit enhancement determinants

Logistic regressions of credit enhancement determinants. For each regression, we report coefficients and elasticities, which are the probability change from a 1 standard deviation change in the independent variable (or from a 0 to 1 change if an indicator variable), while keeping all other variables constant at their means. The main variable of interest is corruption, which we measure in three different ways, and identify in column headings. Specifically, we measure corruption with an indicator variable for state-years in the top quartile of federal convictions; number of convictions per million population; and an indicator variable for State ranking of the quality of state anti-corruption laws greater than or equal to 40. This ranking is produced by the Better Government Association and a higher number corresponds to worse quality of state anti-corruption laws. The remaining independent variables are: “Minority indicator”, which identifies deals in which the lead-underwriter is a minority-owned firm; “GO bond indicator”, which identifies general obligation bonds; “Negotiated bid indicator”, which identifies deals where the underwriter is engaged through negotiated bidding; “Ln(Size)”, the natural logarithm of bond size; “Ln(Maturity)”, the natural logarithm of bond maturity; “Ln(Underwriter mkt. share)”, the natural logarithm of underwriter market share measured as the percentage of total municipal bond value underwritten by that particular underwriter during the year; and “GSP per capita”, the Gross State Product as a proportion of the state’s population. All regressions are inversely weighted by the number of state-year bonds, except the last, which is the non-weighted version of the first regression. All regressions include year and region dummies (not tabulated). Region dummies divide the country into four parts as classified by the U.S. Census Bureau: West, Midwest, South, and Northeast. Robust *p*-values (in parentheses) take into consideration standard errors that are adjusted for within-state clustering.

	Convictions top quart. ind.		Convictions per mill. pop.		BGA rank ≥ 40 ind.		Non-weighted	
	Coeff.	Elast.	Coeff.	Elast.	Coeff.	Elast.	Coeff.	Elast.
Constant	-4.4295** (0.012)		-4.6184*** (0.007)		-5.0299*** (0.000)		-5.7617*** (0.000)	
Corruption measure	0.2522* (0.066)	0.0434	0.0826** (0.032)	0.0297	0.5968*** (0.005)	0.1095	0.1787 (0.175)	0.0298
Minority indicator	0.0483 (0.792)	0.0081	0.0364 (0.841)	0.0061	0.0579 (0.754)	0.0098	0.0454 (0.821)	0.0075
GO bond indicator	0.2233 (0.441)	0.0361	0.2297 (0.431)	0.0372	0.1977 (0.495)	0.032	-0.0188 (0.906)	-0.0031
Negotiated bid indicator	0.3489*** (0.001)	0.0584	0.3442*** (0.001)	0.0577	0.3086*** (0.002)	0.0515	0.3827*** (0.000)	0.0627
Ln(Size)	0.4607*** (0.000)	0.1174	0.4582*** (0.000)	0.1168	0.4595*** (0.000)	0.1168	0.4660*** (0.000)	0.1161
Ln(Maturity)	1.6718*** (0.000)	0.2400	1.6729*** (0.000)	0.2403	1.6684*** (0.000)	0.239	1.4389*** (0.000)	0.2023
Ln(Underwriter mkt. share)	-3.8183 (0.168)	-0.0159	-3.7956 (0.171)	-0.0158	-3.5627 (0.205)	-0.0148	-4.3049** (0.032)	-0.0175
GSP per capita	-0.0175 (0.700)	-0.0200	-0.0177 (0.688)	-0.0203	-0.0030 (0.923)	-0.0034	0.0007 (0.981)	0.0008
Year dummies	Yes		Yes		Yes		Yes	
Region dummies	Yes		Yes		Yes		Yes	
Pseudo-R ²	0.3401		0.3410		0.3441		0.2829	
No. of observations	127,974		127,974		127,974		127,974	

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 6
Choice of underwriter

Regressions of natural logarithm of underwriter market share measured as the percentage of total bond value in the muni (first two and last three regressions) or corporate (third regression) bond markets underwritten by that particular underwriter during the year. Column headings identify the market for which we compute underwriter market share (muni vs. corporate bond markets). The main variable of interest is corruption, which we measure in three different ways, and identify in column headings. Specifically, we measure corruption with an indicator variable for state-years in the top quartile of federal convictions; number of convictions per million population; and an indicator variable for State ranking of the quality of state anti-corruption laws greater than or equal to 40. This ranking is produced by the Better Government Association and a higher number corresponds to worse quality of state anti-corruption laws. The remaining independent variables are: “Minority indicator”, which identifies deals in which the lead-underwriter is a minority-owned firm; “GO bond indicator”, which identifies general obligation bonds; “Negotiated bid indicator”, which identifies deals where the underwriter is engaged through negotiated bidding; “Ln(Size)”, the natural logarithm of bond size; “Ln(Maturity)”, the natural logarithm of bond maturity; and “GSP per capita”, the Gross State Product as a proportion of the state’s population. Regressions either include year dummies (not tabulated), or a “Pay-to-play indicator” variable, which identifies the pay-to-play era (i.e., the bond was issued before April 1994) and a “Time trend”, which counts for the year of the issue, where first sample year takes the value of 0. When using the pay-to-play indicator we omit observations in the months immediately surrounding April 1994 (i.e., March 31 through July 1, 1994). All regressions are inversely weighted by the number of state-year bonds, except the last, which is the non-weighted version of the first regression. All regressions include region dummies (not tabulated). Region dummies divide the country into four parts as classified by the U.S. Census Bureau: West, Midwest, South, and Northeast. Robust *p*-values (in parentheses) take into consideration standard errors that are adjusted for within-state clustering.

	Convict. top quart. ind.	Convict. top quart. ind.	Convict. top quart. ind.	Convict. per mill. pop.	BGA rank ≥ 40 ind.	Non- weighted
	Muni market	Muni market	Corp. market	Muni market	Muni market	Muni market
Constant	0.0024 (0.648)	0.0012 (0.817)	0.1269* (0.065)	0.0023 (0.687)	0.0027 (0.499)	0.0015 (0.664)
Corruption measure	-0.0013** (0.041)	-0.0012* (0.052)	-0.0166* (0.083)	-0.0001 (0.614)	-0.0030** (0.015)	-0.0011* (0.079)
Minority indicator	-0.0187*** (0.000)	-0.0188*** (0.000)	-0.2657*** (0.000)	-0.0187*** (0.000)	-0.0187*** (0.000)	-0.0174*** (0.000)
GO bond	-0.0036*** (0.008)	-0.0036*** (0.009)	-0.0688*** (0.009)	-0.0036*** (0.009)	-0.0035** (0.011)	-0.0029*** (0.001)
Negotiated bid	-0.0024 (0.110)	-0.0024 (0.125)	-0.0491 (0.103)	-0.0025 (0.104)	-0.0022 (0.130)	-0.0029*** (0.008)
Ln(Size)	0.0059*** (0.000)	0.0059*** (0.000)	0.0945*** (0.000)	0.0059*** (0.000)	0.0059*** (0.000)	0.0064*** (0.000)
Ln(Maturity)	0.0015** (0.016)	0.0014** (0.018)	0.0087 (0.451)	0.0015** (0.015)	0.0016*** (0.003)	0.0014** (0.019)
GSP per capita	0.0004*** (0.008)	0.0004*** (0.009)	0.0043* (0.047)	0.0004** (0.013)	0.0004*** (0.004)	0.0004*** (0.006)
Pay-to-play indicator		0.0018*** (0.002)				
Time trend		-0.0001 (0.533)				
Year dummies	Yes	No	Yes	Yes	Yes	Yes
Region dummies	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.219	0.217	0.115	0.218	0.220	0.220
No. of obs.	127,974	125,676	127,974	127,974	127,974	127,974

* significant at 10%; ** significant at 5%; *** significant at 1%

Figure 1

This figure is a partial regression plot of Table 3, regression model 4. The vertical axis is bond yield to maturity and the horizontal axis is convictions per million population. Plotted separately are predicted yields for bonds without credit enhancement (top; solid line with filled diamond markers) and predicted yields for bonds with credit enhancement (bottom; dashed line with unfilled square markers). All variables except for convictions are held constant at their means. Slopes are the regression coefficient on convictions for the top line and the sum of the coefficient on convictions and the interaction between convictions and credit enhancement for the bottom line. Individual points on the lines correspond to various levels of convictions (e.g., the minimum, the median, and so forth).

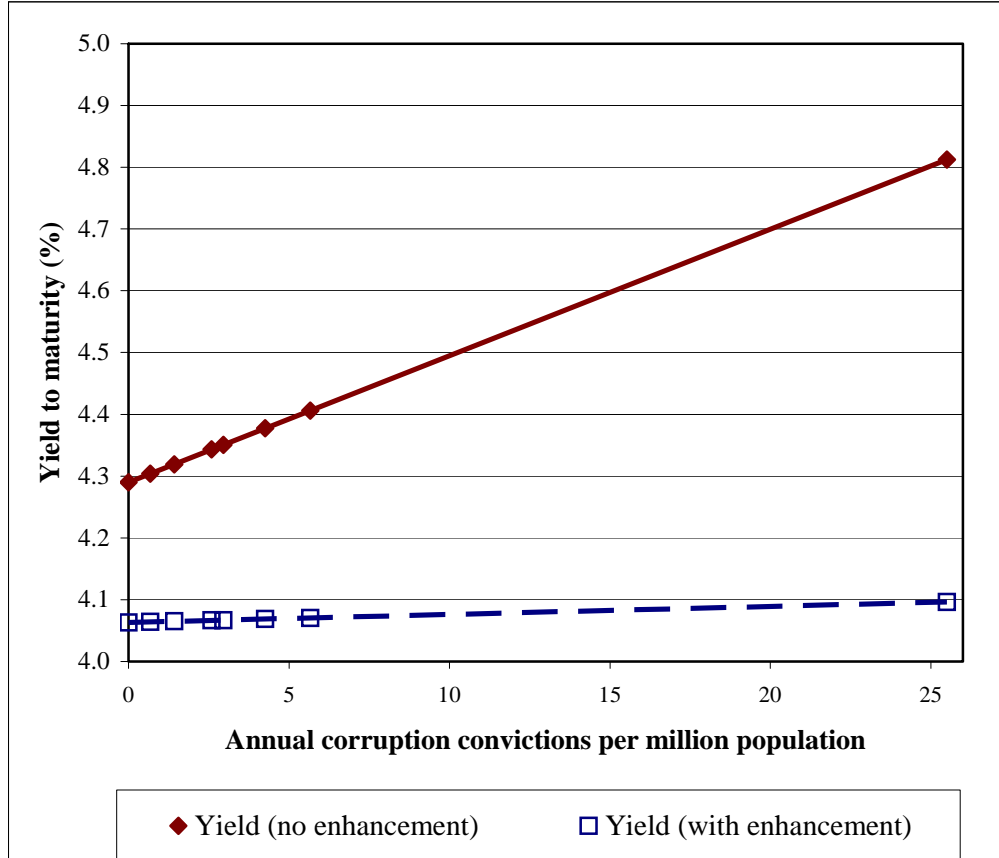


Figure 2

This figure is a partial regression plot of Table 4, regression model 2. The vertical axis is gross spread and the horizontal axis is time (in years). Plotted separately are predicted gross spreads for negotiated deals (top; diamonds) and competitive deals (bottom; circles). All variables except for time and a discrete vertical shift for pay-to-play are held constant at their means. Slopes are the regression coefficients on the time trend variable.

