Trust and Contracts: Empirical Evidence*

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Abstract

How does trust affect contract completeness? If contracting parties were suspicious about each others' reactions to unplanned contingencies, they might pay higher negotiation costs to complete contracts ex-ante. Meanwhile, mistrust might lead to prohibitively high negotiation costs and less complete contracts (or no contracts altogether). Using a unique sample of U.S. consulting contracts, text-based-analysis measures of contract completeness, and a shock to trust between shareholders/managers (principals) and consultants (agents) staggered across space and time, we find lower trust increases contract completeness. The clauses added to contracts after trust drops related to potential observable and verifiable misbehavior by agents or principals.

JEL classification: D86, D91, J33, L14, Z10.

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I Introduction

The design of principal-agent contracts shapes economic activity as studied in disparate fields such as accounting, finance, labor economics, and industrial organization. In particular, the causes and consequences of contract incompleteness—the fact that parties do not contract on all verifiable contingencies—has been an important focus of contract theory (see Hart and Moore (1988, 1999)). Contract incompleteness imposes renegotiation costs (Segal (1999)), might induce costly access to courts (Lerner and Schoar (2005)), delay economic activity, and limit financial flexibility (Tirole (2006)). At the same time, incompleteness might be inevitable due to parties' limited ability to foresee future contingencies (Tirole (2009); Blankespoor et al. (2020)). Despite a large theoretical literature, our empirical understanding of the drivers of contract completeness is limited (Iyer and Schoar (2015); Schoar (2016)). Deepening such an understanding extends our application of incomplete contract theory with information frictions between corporate insiders and outsiders (e.g., Armstrong et al. (2010); Costello (2013); Christensen et al. (2016)).[1]

The extent of trust among parties—the subjective probability principals and agents attach to future predatory actions by the counterpart in the face of unplanned events (Guiso and Makarin (2020))—seems a natural potential driver of contract completeness (Bottazzi et al. (2016); Ederer and Schneider (2019); Huang and Hilary (2021)). Theoretically, the effect of lower trust between parties on contract completeness might act through two opposite channels. On the one hand, if both the principal and agent share a belief that the other party is unlikely to engage in ex-post predatory behavior, they may avoid costly negotiations to plan for contingencies, such as the unauthorized use of confidential information. In contrast, if trust drops (Gurun et al. (2017)), each party might insist on planning for such contingencies ex ante, thus enhancing contract completeness (e.g., Dye (1985)). On the other hand, a drop in trust might increase negotiation costs to a prohibitively high level, which would lead to failing to stipulate a contract altogether

[1]For instance, see Chiappori and Salanié (2003); Eigen (2012); Antràs and Foley (2015); Ganglmair and Wardlaw (2017); Buchak (2016); Iyer and Sautner (2018); Iyer and Schoar (2020); Gennaioli et al. (2020); Akey and Appel (2021); Hui et al. (2024).

(e.g., Tirole (2009)).

This paper proposes an empirical test of whether and how mutual trust among parties affects contracting (see Gennaioli et al. (2020) for another related work). The first challenge to address this question is observing a large, representative, and homogeneous sample of principal-agent contracts in which contingencies can be defined and measured. The second challenge is isolating a quasi-exogenous source of variation in the trust between prospective principals and agents. We tackle these challenges by introducing novel data and measures of contract completeness and by exploiting a quasi-exogenous shock to trust among contracting parties in the context of consulting contracts between a principal (i.e., firm's shareholders/management) and an agent (i.e., consultant).

Our sample consists of all consulting agreements US public firms are required to report to the Securities and Exchange Commission (SEC) through mandatory filings of material interests. For each contract, we extract the principal and agent identities, whether the agent is an individual consultant or a consulting company, the contracting date, the state of business, duration, payment amount and type, and the full text of the contract. Figure A.1 is an example of a contract in our sample and Table 1 reports a few examples of client-consultant pairs, consultants' identities, and the types of consulting services provided, if available. Consistent with earlier research (e.g., Bruhn et al. (2018)), most clients in our sample are listed firms of small or medium size and consultants are either individuals or employees in small consulting firms.[2]

Our first contribution is the creation of empirical measures of contract completeness, which can be used broadly by future research in empirical contract theory (see Section III for details). Our main measure is based on the number of topics contracts cover. In robustness analysis, we integrate this measure by analyzing contracts' structure in terms of unique words and length of contract clauses. The number of topics, words, and length are meaningful proxies of completeness because contract clauses, contrary to narrative texts such as news articles, display standardized structures and semantics. And, each word has a specific legal meaning that identifies one concept contrary to the use of synonyms and figures of speech in narrative texts. Adding a topic to a contract means planning on

[2]No reported contracts were signed between listed firms and Big Five companies. Our study is therefore silent on how trust affects the incompleteness of contracts with auditing firms.

additional contingencies.

To tackle the second empirical challenge—detecting a shock to trust between the prospective parties of consulting contracts—we exploit the 2002 Enron scandal. Arthur Andersen LLP (AA) provided auditing service to Enron and was one of the five largest auditors worldwide (*Big Five*) until 2002, when it was convicted by US courts for destroying documents related to the Enron scandal and ceased operations. This historically unprecedented accounting scandal not only echoes widespread concerns that auditing firms' audit quality has been threatened (e.g., Harris (2014); Harris (2015); Kowaleski et al. (2018)), but has also been shown to increase suspicion, scrutiny, and fraud detection for private and public firms that were more exposed to AA's auditing activities before the scandal.[3]

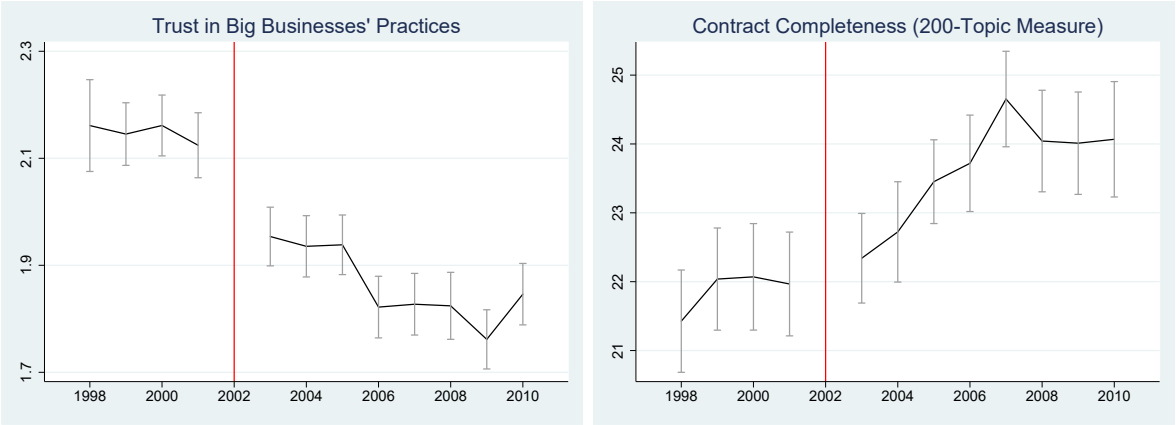
Despite its auditing-specific origination, anecdotally the Enron scandal reduced the general public's trust in the practices that large U.S. corporations undertake beyond specific functions such as accounting and auditing.[4] We provide direct and systematic evidence that this drop in the public's trust in big firms' business practices in general after the AA scandal was stronger in US states that had more AA clients before the scandal, and hence were likely more exposed to it.

The drop in mutual trust between principals (i.e., shareholders/managers) and agents (i.e., consultants) induced by the scandal could affect contracting in several ways. First, shareholders' lower trust in business practices, and the consequent threat of selling their shares, force managers to take an extra level of caution when negotiating contracts with external consultants to diminish the probability of ex-post opportunistic behavior, such as exploiting valuable private information about the firm or luring their clients. This channel operates irrespective of whether the scandal reduces managers' trust in consultants, i.e. whether managers' perceived likelihood of ex-post predatory behavior on the part of

[3]The literature on this fraud-detection mechanism due to the demise of AA includes Nagy (2005), Krishnamurthy et al. (2006), Krishnamurthy et al. (2006), Chen and Zhou (2007), Krishnan (2007), Krishnan et al. (2007), Kohlbeck et al. (2008), Dyck et al. (2023), Giannetti and Wang (2016), and Ozdagli (2018), among others.

[4]During their partnership's life, AA auditors at several regional offices failed to detect, ignored, or approved accounting frauds for large clients paying lucrative consulting fees, including Enron and WorldCom. For how the American public reacted toward the Enron crisis, see, e.g., "Question and Answers About Enron"; *Gallup News*, February 14, 2002; "Public Confidence in the Wake of Enron"; *Gallup News*, February 19, 2002.

Figure 1: **Trust and Topics Covered in US Consulting Contracts**



The left panel reports the average extent of trust in big businesses by a representative US population based on a scale from 1 (low trust) to 4 (high trust) surveyed yearly between 1998 and 2010. The right panel reports the average number of topics in our sample of consulting contracts signed in the United States between 1998 and 2010 based on SEC reports by US-listed firms as well as private firms that issue public debt instruments. We describe in detail the definition and construction of this measure in section III.

consultants has increased after the scandal, because small shareholders are more sensitive to news about scandals and more willing to sell their shares if a predatory event happens relative to before the scandal. Second, if the scandal also lowers managers' trust in agents, managers will be willing to complete contracts because they think that the likelihood of ex-post predatory behavior has increased. Third, consultants themselves might become more distrustful of firms' management and impose the negotiation a wider spectrum of clauses to define explicitly the actions that might escalate the risk of an infringement lawsuit.

Before we move on to describe our empirical strategy, we depict the univariate time-series evidence that motivates our subsequent analyses in Figure 1. The left panel plots the time series of trust in big business practices among US households—who are shareholders of public firms through their stock holdings—based on the Gallup Trust Survey. Average trust in big business practices dropped in 2002 and did not rebound in the subsequent years. At the same time, the right panel shows that the completeness of consulting contracts has been increasing since 2002 based on the number of topics the average contract covers. Figure 2 documents the same fact when using proxies of completeness based on the number of words and sentences in contracts.

Although interesting as a motivation, the evidence in Figure 1 could capture any time-varying shocks contemporaneous to the AA scandal that is also correlated with trust and with contracting practices. A prime example is the implementation of the Sarbanes-Oxley Act (SOX) after 2002. To tackle this issue, we turn to the spatial variation in exposure to the AA scandal. We show that a higher state-level share of public firms audited by AA in 2000 is associated with a larger drop in trust in big business practices after 2002. We use this differential pre-scandal spatial exposure to the shock in a difference-in-differences (DiD) empirical strategy. We compare the completeness of contracts managers sign on behalf of shareholders before and after the shock and across states that had differential pre-exposure to the shock.[5]

Our difference-in-differences analysis confirms that lower trust among parties increases contract completeness. After 2002, our preferred measure of completeness—the number of topical areas contracts cover—increased by 10% more in states with a higher share of AA-audited public firms relative to other states. Results are similar for all our measures of completeness and across several robustness tests.

A set of falsification tests corroborate our interpretation: The state-level shares of firms that were audited by other Big Five are unrelated to trust in business practices or contract completeness, which rules out systematic shocks to the consulting services industry, such as potential innovation in consulting contract boilerplate templates (e.g., see Choi and Gulati (2004); Murfin and Sun (2023)). Moreover, trust in other institutions that shape contracting practices, such as the judicial system and banks, did not change differentially across states after 2002. And, the completeness of consulting contracts with firm insiders, such as C-suite managers, whom shareholders trust by revealed preferences (otherwise, shareholders would have sold their stocks, as Giannetti and Wang (2016) show) does not change over time.

By construction, our analysis is based on the contracts that are signed and disclosed and hence the econometrician observes as being part of the sample. One might worry that the AA scandal affected the selection of contracts in the sample because some

[5]We show that our results are similar when we exclude all AA client firms after the scandal, which were forced to write new contracts with other consultants and which belong to selected industries (Nelson et al. (2008)).

types of contracts that were routinely signed before the scandal might cease to exist afterward either because those relationships stopped being contracted upon or firms stopped disclosing them. New federal disclosure requirements such as those in SOX cannot drive our results, which exploit cross-state variation, by construction. Nonetheless, we show that the average number of disclosed contracts, of disclosing firms, and of contracts disclosed per firm did not increase after 2002 or across states. The types of disclosing firms and consultants involved in disclosed contracting relationships do not change either, which limits the concern that the nature of contracted relationships changed due to the AA scandal. Moreover, the extent of civil lawsuits involving contract violations did not change across states and over time, which dismisses a role for changing legal standards due to increased litigation in high-AA states.

In the last part of our analysis, we ask which contract clauses appear more often in lower-trust environments. The list includes confidentiality and indemnification clauses, termination clauses, and non-compete clauses (NCCs), which start to appear only in the states in which they are enforceable. The nature of these clauses stresses the intent to define verifiable states of the world under which agents are in breach of contract, which increases contract completeness rather than merely increasing contract complexity.

We cannot assess the role of trust on repeated contracting within the same relationship (Malhotra and Murnighan (2002); Lumineau (2017)) because we only observe a small number of contracts between the same firm and the same consultant before and after 2002 once we exclude mere deadline updates. Future research should analyze the dynamic effects of trust on contracting both theoretically and empirically.

II Conceptual Framework and Data

In this section, we first introduce the conceptual framework that guides our empirical analysis. We then discuss the construction of our sample of consulting contracts as well as the measures we use to map the conceptual framework into empirical tests we can bring to the data.

A. (In)completeness and Trust as Expected Probability of Predatory Behavior

Our analysis is based on the notion of *functionally complete contract*—a contract to which parties cannot add any contingency either because the occurrence of such contingency would not be verifiable ex-post or because it would be too costly to describe the state of the world under which such contingency arises (Eggleston et al., 2000). A functionally incomplete contract is thus a contract to which ex-post verifiable contingencies can be added.

Incomplete contracts can arise because of transactions costs (Williamson (1985); Hart and Moore (1988)), such as the costs of negotiating contract clauses, or because the parties cannot foresee and describe all future verifiable contingencies (Maskin and Tirole (1999); Tirole (2009)) and/or enjoy mutual benefits from avoiding to bargain on all potential contingencies at the onset of the contractual relationship (Crocker and Reynolds (1993); Halonen-Akatwijuka and Hart (2013)).

Limited foreseeing of potential future contingencies is unlikely in our setting. We show that more complete consulting contracts cover standard restrictions to agents' actions, such as the punishment for stealing of proprietary information and NCCs, all of which contingencies have been codified for centuries (e.g., see the notion of *actio servi corrupti* in Ancient Roman Law). A substantial option value from postponing the negotiation of some contingencies is also unlikely because of imbalanced bargaining power between principals (large public firms) and agents (individual consultants).

Ultimately, transaction costs are the most plausible driver of contract incompleteness in our setting. If the parties face transaction costs when negotiating contracts, the extent of completeness will depend on the trade-off between the costs and benefits of negotiating each additional clause.

Mutual trust between principal and agents should therefore contribute to determining the extent of contract completeness (e.g., Gennaioli et al. (2020)). On the one hand, principals' trust in the agent, that is, the subjective expected probability that a negative state of the world caused by the agent might arise, is important to determine if the

principal will want to plan for such a potential negative state. For instance, consider the issue of agents dealing with proprietary information. If the principal trusts the agent—she thinks the probability the agent might steal proprietary information is minimal—she might prefer to not engage in endless negotiations with the agent about which uses of the data are accepted and in what contexts. By contrast, if the principal did not trust the agent, she would be inclined to pay the costs of negotiating confidentiality clauses.

On the other hand, agents' trust in the principal should also matter. Agents, too, might request the negotiation of clauses (e.g., confidentiality) that specify which actions represent wrongdoing within the relationship because they might worry about specious litigation by the principal during and after the contractual relationship. Another example relates to compensation: the value of cash payments does not depend on the principal's action after the contract is signed, whereas the value of equity compensation does. If agents' trust in the principal drops, agents might prefer cash payments or, under equity compensation, they might require that details about the valuation and situations under which compensation is in the form of firm equity are specified in the contract, which increases contract completeness.

If, however, the costs of negotiating contingencies become prohibitively high due to a drop in trust between contracting parties, the aforementioned predictions might be reversed: if the parties find it too costly to complete contracts but still valuable to contract, they might end up signing more incomplete contracts. If the costs of contracting are higher than the benefits of entering in the contractual relationship, the parties might decide to not sign a contract altogether.

B. Constructing the Sample of Consulting Contracts

We draw our sample of consulting contracts from the external-service material contracts US companies file with the US Securities and Exchange Commission (SEC) from 1994 to 2015.[6] The firms whose contracts we observe are regulated by the SEC, that is, they are either public firms or private firms that issue public debt instruments in the US.

Regulation S-K of the Securities Act of 1933 mandates that publicly filing companies

[6]Our sample stops in 2015 to avoid censoring when measuring contract renegotiation.

include all "material contracts" as exhibits in SEC filings (see Section 10 (ii)(b) of Regulation S-K). The term "material" in SEC regulations limits disclosure to those contracts for which there exists a high probability that a reasonable investor would attach importance to the contract in determining whether to buy or sell the registered securities (e.g., Overdahl (1991)). Material contracts must be filed if they are (1) not in the ordinary course of business or (2) within the ordinary course of business and the filer's business is substantially dependent on the contract during the filing period (e.g., Verrecchia and Weber (2006); Costello (2013); Hui et al. (2024)).

Based on this institutional background, our sample of consulting contracts has the following three features. First, client firms disclosing contracts are relatively small and growing firms (e.g., Bruhn et al., 2018). Second, in about 70% of our sample contracts, consulting services were performed by individual consultants. Third, because public firms are required to disclose auditor engagement and audit fees in their proxy statements, our sample does not include contracts between firms and independent registered public accounting firms (i.e., external auditors).

Firms can file material contracts in three ways—under Exhibit 10 of 8-K forms or Exhibit 10 of the annual or quarterly financial reports (i.e., 10-K and 10-Q filings). Form 8-K is the "current report" companies are required to file within 4 business days from the occurrence of major events about which shareholders should be informed rather than at regular intervals like the other two disclosure vehicles. In all three cases, the full contracts are attached and made public. Timing is the main difference among these three disclosure vehicles. If the firm chooses to disclose the contract in an 8-K filing, the information is disseminated when the contract is signed. Under 10-Q or 10-K filings, instead, the information is disclosed only at the end of the quarter or fiscal year. Firms might have unobserved strategic motives when choosing the disclosure vehicle. For this reason, in all our multivariate analyses we control for the form of disclosure and we replicate our results separately across disclosure vehicles. These analyses dismiss that our results might be related to the choice of disclosing vehicle.

To identify and access consulting agreements, we obtain hyperlinks to the main Edgar webpages for all 8-K, 10-K (and 10-KSB), and 10-Q (and 10-QSB) filings and their

amendments filed with the SEC between 1994 and 2015. To do so, we use an automated Python program to crawl the SEC's index files and collect all hyperlinks related to these filings, and then download the filings.[7] We use the "List of Filings Exhibits" file in the SEC Analytics database to identify Exhibit 10 sections within each form, and then use a text parsing tool in Python to extract the corresponding 334,988, 266,198, and 304,674 Exhibit 10 sections as attachments in 8-K, 10-K, and 10-Q filings.

Note that 8-K filings happen within a few days from an even the firm finds material enough to be disclosed at the time it happens, whereas 10-K and 10-Q filings are due at pre-specified times, i.e. annually and quarterly, respectively. Because the timing of disclosure is endogenous for 8-K filings, in our multivariate analyses in which contract completeness is the outcome variable we add dummies for whether the contract was disclosed as an appendix of an 8-K filing among the controls.

We narrow the scope of our search to consulting agreements by identifying the titles of all Exhibit 10 sections of each filing that include at least one of the following terms: "consulting," "consultant," "consultation," "advice," and "advisory." Each contract has a unique firm identifier (*cik*). In order to link contracts to the originating firm and hence match it to firm-level characteristics, we obtain the global company key (*gvkey*) from the "List of Filings Exhibits" file.

We check manually each contract to ensure that none of the following cases enters the sample: (i) amendments to contracts due to renegotiation, which would not represent full contracts and hence would bias our measures of completeness (we code the presence of renegotiations as a separate variable); (ii) duplicated contracts, which are identically reported through more than one SEC form filing; (iii) contracts that do not include the year in which the agreement was signed, for which we would not be able to assign a treatment or control condition based on our empirical strategy; and (iv) contracts for which we cannot obtain the *gvkey* through the "List of Filings Exhibits," for which we do not observe firm-level characteristics.

This procedure leaves us with 6,081 distinct consulting agreements, of which Figure A.1 reports an example. We exploit the richness of these data to extract several

[7] We can share the Python script code for these steps upon request.

characteristics of both principals and agents using an automated process supplemented with manual checks. For principals, we obtain information about listing status, firm name, *gvkey*, filing date, contracting date, form filed with the SEC (8K, 10K, or 10Q), business state, state of incorporation, the Standard Industrial Classification (SIC), and the firm's headquarters zip code. For public firms, we use *gvkey* codes to obtain firm characteristics from the Compustat/CRSP database. For agents, we obtain the name, whether the consultant is a firm insider (previous CEO, previous director, or current employee), whether the consultant is an independent contractor or a firm, and the zip code associated with the consultant's reported address.

As far as contract terms are concerned, we extract information on the effective dates of the contract, the contract's duration (in months), the amount of cash payment and frequency of pay, whether grants of stocks and options apply, whether the contract includes a non-compete clause, a confidentiality clause, or an arbitration clause, and the choice of state for governing laws.

III Measuring Contract Completeness

Before we can tackle our question, we need to define the mapping of the concept of contract completeness into a variable we can measure in the data. Ideally, we would measure the number of contingencies the parties agree to include in the contract *and* whether these contingencies are verifiable. The number of contingencies captures the complexity of the contract, but higher complexity makes a contract more complete only if the additional contingencies are verifiable.

To conceptualize the difference between contract completeness and complexity, consider a clause that we show tends to be added to contracts in low-trust environments—non-compete agreements, whereby the agent agrees to commit to not compete with the principal in the principals' areas of activity once the contractual relationship is over, for instance by luring clients. Adding a non-compete agreement increases both contract completeness and complexity, because whether an agent engages in competition against the principal is verifiable.

Based on these considerations, we propose two empirical approaches to capture contract completeness. The baseline approach is based on complexity, under the assumption that a more complex contract is also more complete as long as at least one of the additional contingencies is verifiable. For this approach, we construct broadly-applicable and homogeneous measures of completeness that can be applied to empirical contract theory questions beyond our study.

To corroborate that this approach captures completeness, and not just complexity, we then propose a second empirical analysis that focuses on specific clauses. We detect whether specific clauses that refer to verifiable contingencies, such as the non-compete agreements discussed above, appear in each contract irrespective of contracts' length and complexity.

Both approaches build on the use of textual analysis in finance and accounting research, as pioneered by Hoberg and Phillips (2010), Hoberg and Phillips (2016) among others and surveyed recently by Loughran and McDonald (2016) and Gentzkow et al. (2019).

A. Topic-Modeling Measures of Contract Completeness

Our baseline measure of contract completeness is the number of topics contracts cover. By construction, more topics mean that the contract covers more future potential contingencies and hence is more complete as long as at least one of the additional contingencies is verifiable. This property is especially true for contracts, relative to prosaic texts, news, and other narrative forms, because contracts use homogeneous and standardized structures and language.

The main challenge to counting the number of topics in contracts is defining a consistent, coherent, and systematic definition of topics and a methodology to assign groups of words to alternative topics. To tackle this challenge, we follow a recent wave of research in economics and finance that uses state-of-the-art textual-analysis techniques (for instance, see Kelly et al. (2021) and Acikalin et al. (2022), among others).

Specifically, we build on the Latent Dirichlet Allocation (LDA) first developed by Blei et al. (2003). The LDA reduces the dimensionality of linguistic data from words to

topics, based on word co-occurrences within a same document. LDA uses a statistical generative model to imitate how a human being writes a contract. In particular, LDA assumes that each word in a contract is generated in two steps. In the first step, LDA assumes that each contract has its own topic distribution. A topic is randomly drawn based on the contract's topic distribution. In the second step, LDA assumes each topic has its own word distribution. A word is randomly drawn from the word distribution of the topic selected in the previous step. LDA repeats these two steps word by word to generate a contract. The algorithm discovers the topic distribution for each contract and the word distribution of each topic iteratively, by fitting this two-step generative model to the observed words in the contracts until it finds the best set of variables describing the topic and word distributions.

To analyze the topic structure of consulting contracts, we use the universe of 6,081 contracts and 1,203 amendments filed with the SEC from 1994 to 2015.

Our procedure consists of two steps. In the first step, the LDA algorithm analyzes the text of the full universe of contracts to identify common topics. Each topic is a matrix that contains two types of elements—a set of words that the procedure identifies as related to each other based on systematic co-occurrence in the corpus of contracts as well as the probability that each word is semantically related to the other words within the topic.[8]

Figure 3 provides a visual representation of two sample topics among the ones the LDA identifies in the first step of the procedure. Each graph in Figure 3 is a cloud representation of the two elements of each topic—the words that are related enough to constitute a topic and the probabilities attached to each word (captured by the font size).

Consider the topic in Panel A, which we label "Arbitration to solve controversies between parties." The vast majority of the words that enter this topic are related to the procedures to be used in case of controversies between the principal and the agent. The words with the highest probability of belonging to this topic are "arbitration" and "arbitrator," which intuitively suggests that several contracts resort to arbitration for the solution of potential future controversies. Other forms of resolution seem less likely but

[8]The LDA procedure requires an assumption about the optimal number of topics to gauge from a corpus of texts. We follow Blei et al. (2003) and choose the number that minimizes their *perplexity score* locally, which is 200 (see Figure A.2 of the Online Appendix).

still present in some contracts, as is evident from the words "trial," "tribunal," and "judge" showing up with lower probabilities. Casual perusal of the other words that enter the topic seems to suggest that the LDA is effective. Most words that enter the topic with high and medium probability relate to controversies, such as "controversy," "jurisdiction," "claims," "damages," "breach," "provisions," "interpretation," "enforceability," and many others.

Of course, not *all* the words the procedure identifies will necessarily and without doubt refer to the topic. Like any other measurement exercise, the identification of topical words must also display some amount of measurement error. Searching whether words barely related to controversies are assigned a low probability is another way to assess our procedure. For instance, the term "san" in the northern part of the cloud, which is attached a low probability, seems barely related to controversies.[9] A second caveat to keep in mind is that some of the words might not be uniquely related to that specific topic. For instance, the words "writing," "county," or "hereof" could be plausibly found in several other topics of a contract.

Panel B of Figure 3 shows the words and probabilities that constitute another topic the LDA analysis identifies, which we label "Relationship to company's board members." The qualitative assessment we discussed above applies very similarly to this topic as well as to the other topics the procedure identifies. In Section VI, we discuss in more depth additional topics and especially the topics that appear with increasing frequency in contracts signed in states that are treated based on our difference-in-differences strategy.

Overall, the ability of the LDA to identify meaningful topics in the universe of contracts depends on its ability to select words that relate to a topic as well as to attach high probabilities to the words that are most related to the topic and the examples in Figure 3 support the viability of the procedure in our sample of consulting contracts.

Note that the LDA procedure requires an assumption about the optimal number of topics that should be identified in the available corpus of contracts. To inform this assumption, we compute the *perplexity score* proposed by Huang et al. (2018).[10] As a

[9]Possibly, the LDA selects this term because several contracts might report the city in which controversies should be solved, and California and Texas are two states in which we observe many contracts and in which the term "san" is commonly part of the name of several large cities.

[10]For a definition and discussion of the perplexity score, please see page 2851 of Huang et al. (2018).

criterion, we use the number of topics that minimizes the perplexity score locally, which is 200 topics in our universe of contracts (see Figure A.2 of the Online Appendix). This value means that the procedure isolates the most common 200 recurring topics in the universe of contracts. To verify that this assumption is immaterial to our results, for robustness we replicate all our results when setting a lower value of 100 optimal topics and a higher value of 300 optimal topics.

The second part of the procedure computes the number of topics (among the 200) each contract includes.[11] We consider each sentence of each contract that enters our analysis. The procedure analyses the words in each sentence and assigns the sentence to one of the 200 topics based on word similarity. We then sum up the number of unique topics covered by each contract.

The number of unique topics in each contract, which is a natural integer bounded between 0 and 200, is our baseline measure of contract completeness.

B. Count Measures of Contract Completeness

The most appealing feature of the topic-modeling-based measures is their ability to capture multiple features of contracts at once consistently and objectively. At the same time, one might be concerned that the LDA method we use is not transparent enough, or that other contract features might also proxy for completeness.

To tackle these concerns, for robustness purposes we also propose proxies for completeness that are based on the count of words and sentences in contracts. Intuitively, the longer is a contract, i.e. the more the sentences and words are in the contract, the more likely is the contract to discuss more contingencies, because consulting contracts have homogeneous structures and use homogeneous semantics. Differences in contract length are unlikely to capture different writing styles, which instead is the case with news or other narrative texts.

We propose three count-based measures of completeness—(i) the number of sentences; (ii) the number of words; and (iii) the number of unique words in each contract. We

[11] We exclude the shortest contracts when computing these measures, because we do not have enough sentences to meaningfully apply our textual-analysis procedure, but we will analyze the subsample of very short contracts in a robustness test below.

construct these measures using textual-analysis algorithms that simply count the words and sentences in contracts.

C. Contract Completeness: Time Series and Cross Section

Our working sample consists of 6,081 consulting contracts, with an average of 3.3 contracts per firm and a median of 2 contracts per firm. Table 2 reports summary statistics. As Panel A indicates, about one third of the contracts (N=1,931) are in the control period, between 1994 and 2002 included (9 years), whereas roughly two thirds (N=4,150) are after the AA scandal—between 2003 and 2015 included (13 years).

In Panel B of Table 2, we describe the sample at the contract level, which is our unit of observation. About two thirds of contracts are signed with agents outside the firm (N=4,067). Our baseline analysis focuses on contracts with outsiders but we propose a falsification test using contracts with insiders, such as former and current executives and board members, to explore the role of intraorganizational trust (e.g., Garrett et al. (2014)). In addition, an average contract is signed in a state in which *Big Five* auditing companies have homogeneous state-level market shares, each serving between 11% and 15% of local listed companies.[12]

We then summarize our main outcome variables—the measures of contract completeness defined above. *Completeness 200* is the topic-based measure when the LDA assumes an optimal number of 200 topics in the universe of contracts used to define topics. The average contract covers slightly less than 23 topics, but the variation is substantial, ranging from a minimum of 3 topics to a maximum of 71 topics. The median is close to the mean, which suggests that the distribution of topics is barely skewed in any directions. The distributions of the measures that assume 100 or 300 optimal topics are quite similar. For the count-based proxies of completeness, the average number of words is 1,168, and 465 of these words are unique. This difference justifies using both measures in the analysis. Also, the average contract has 69 sentences.

As far as other contract features are concerned, *Non-compete* is a dummy that equals

[12]These shares do not sum up to 1, because of smaller auditing firms and individual consultants not being affiliated with any *Big Five*.

1 if the contract includes a non-compete clause, which is the case for about 30% of the sample. A subset of contracts (N=4,949) express their duration—21 months on average. Also, 3.5% of the contracts are renegotiated and one third are disclosed.

For all the empirical tests that require us to focus on the contracts signed between public firms and external consultants, we consider the subsample of 2,772 contracts that satisfy these criteria, whose contract-level descriptive statistics are in Panel C of Table 2.

Because our contract sample is novel, in the Online Appendix we summarize the distribution of contracts across US industries (Table A.1) and across US states (Table A.2). Our baseline analysis uses fixed effects to keep variation constant across these (interesting) sources of variation, which are therefore irrelevant to our empirical results.

IV Shock to Trust in Big Business Practices

In this section, we validate the drop in trust in the difference-in-differences analysis.

A. Enron Scandal and the Demise of Arthur Andersen

Enron Corporation was a Texan public company that was involved in one of the largest accounting fraud scandals in history (the Enron scandal), which led to its bankruptcy. According to trial evidence, for years the company had hidden billions of dollars in debt from failed projects using accounting loopholes, special purpose entities, and poor financial reporting.

Arthur Andersen LLP (AA)—Enron's auditor—did not report these misguided practices. On March 5, 2001, Ms. Bethany McLean raised doubts about Enron's financial accounts in a journal article and on October 16, 2001 Enron announced major restatements to their end-of-fiscal-year accounts for the years between 1997 and 2000. Soon thereafter, on November 30, 2001, Enron filed for bankruptcy.

AA was charged of negligence and fraudulent behavior. On January 17, 2002, Enron dismissed AA accusing them of fraudulent auditing and the destruction of documents that would prove AA's misbehavior. For the latter accusation, AA was found guilty of obstruction of justice. On August 31, 2002, amid the scandal that followed the court's

ruling, AA surrendered its CPA license and shrank its business activities by laying off about 85,000 employees. Even though the US Supreme Court overturned AA's conviction unanimously in 2005, the scandal and loss of reputation loomed so large that AA stopped most of their operations. Part of AA's activities continued after the restructuring under the name Accenture.

B. Variation in Exposure to the Scandal Across US States and Trust

The raw-data time-series evidence in Figure 1 is not sufficient to conclude that the AA scandal was the only shock that lowered trust in business practices after 2002, which in turn affected contracting practices. On the one hand, other contemporaneous shocks might have contributed to the drop in trust and/or change in contracting practices. On the other hand, the scandal might have affected other aspects of principals and agents' preferences and beliefs, which in turn might have changed contracting practices.

To tackle these concerns, we design a difference-in-differences empirical strategy that exploits cross-sectional variation in the extent of exposure to the AA scandal by principals and agents—the state-level share of public companies that were audited by AA in 2000. Spatial variation in the salience of scandals based on pre-scandal exposure has been used by other recent studies on cultural norms and financial decisions (e.g., see Gurun et al. (2017); D'Acunto (2019); D'Acunto et al. (2019)).

This strategy builds on the notion that after 2002 frauds were more likely to be detected and prosecuted on public firms audited by AA before the scandal. One channel for higher detection was the incentive of new auditors replacing AA to report misbehavior during AA's tenure. Existing studies document that higher fraud detection led to stock sales and lower stock-market participation by local investors, which research finds relates to investors' trust (e.g., see Guiso, Sapienza, and Zingales (2008); Gurun et al. (2017); D'Acunto et al. (2019); Hayes et al. (2021)). To assess this channel more directly than in previous work, we estimate the following linear specification using responses for waves of

the Gallup Trust Survey between 1990 and 2015:[13]

$$Trust_{s,t} = \alpha + \beta \ Treated_s \times After\ 2002_t + X'_{s,t}\delta + \eta_s + \eta_t + \epsilon_{s,t}, \tag{1}$$

where $Trust_{s,t}$ is the share of respondents who report a high degree of trust in big business practices in state s in year t;[14] $Treated_s$ is a dummy that equals 1 if state s is in the top 25% of US states based on the share of public companies that were audited by AA in 2000; $After\ 2002_t$ is an indicator variable that equals 1 for the period 2003-2015, and zero for the period 1994-2002; $X_{s,t}$ is a vector of averaged respondent-level characteristics including the logarithm of age, race dummies, a dummy for whether the respondent identifies as a Republican voter, a dummy for whether the respondent is Protestant or Jewish, a dummy for male respondents, married respondents, and respondents in the top bracket of income; η_s and η_t are full sets of fixed effects for states and years. Panel D of Table 2 reports descriptive statistics of the above variables at the state-year level.

We depict the variation of the share of AA-audited public firms across states as of 2000 in Figure 4. For brevity, we only report specifications using the non-linear dummy variable described above, but all our results are similar if we define *Treated* as the continuous version of the state-level share of public firms audited by AA.

We report the results from estimating equation (1) in columns (1)-(3) of Table 3. Column (1) shows that trust in big business practices decreased by one fifth of a one-standard-deviation of trust in the full sample in states whose share of AA clients among local public companies was higher before the scandal. This result is estimated after accounting for US-wide economic shocks by adding year fixed effects. In columns (2)-(3), we add state fixed effects to ensure that time-invariant state-level characteristics do not drive our results and averaged respondent-level characteristics to account for observable time-varying state-level drivers of trust. The results are quantitatively and qualitatively similar.

[13] Giannetti and Wang (2016) use the same survey data to document a positive correlation between state-level yearly fraud revelation and the change in respondents' trust in big business practices.

[14] Specifically, we average dummies for whether a respondent in state s in year t reports a value of trust in big business practices equal to 3 ("a lot") or 4 ("a great deal").

B.1 Parallel-Trends Assumption

Interpreting the results from these double-differences specification causally requires a parallel-trends assumption, whereby the econometrician assumes that, absent the Enron scandal, the trends of trust in big business practices would have been parallel both before and after 2002 across states with a higher or lower share of AA-audited firms. Although this assumption is untestable given that the counterfactual trends for treated states after the scandal are unobservable, at a minimum we can test if trends were parallel across groups of states before 2002. We do so by estimating a version of equation (1) in which we include interactions between the treatment variable and each period dummy variable indicating years before and after the Enron scandal, which we compare with the post-scandal period, i.e. the period in which we argue that trends diverged across groups of states.

We estimate following linear specification in which observations are weighted by the number of survey respondents in each state-year unit:

$$Trust_{s,t} = \alpha + \sum_{\tau=-4}^{6} \beta_\tau \times Treated_s + X'_{s,t}\delta + \eta_s + \eta_t + \epsilon_{s,t}, \tag{2}$$

where $Period_\tau$ is a dummy variable that indicates the τth period listed in the left column. We create two-year periods except for the first few years of our sample (1990-1995), in which the number of contracts disclosed each year is low. The interactions with 2002 is the omitted category and serves as the base period. Thus, our estimated coefficients represent the change in the difference between treatment and control groups relative to 2002.[15]

Table 4 reports the results. We fail to reject the null hypothesis that before 2002 trust in big business practices did not differ across states with a higher or lower share of AA-audited public firms at the time of the scandal. The trends instead diverge significantly after 2002. However, trust in treated states indeed declined relative to control states

[15]The Gallup yearly surveys carried out in the first few months of a year. Therefore, outcomes of survey in year t corresponds to participants' response to information observed by the end of year t-1. Because the AA scandal was fully revealed on August 31, 2002, and because state-level fraud revelation based on the pre-existing share of AA-audited firms took another several years afterwards, we set 2002 as the base period.

even during periods prior to 2002. The pattern raises concerns that there might exist slow-moving secular trends that differ systematically across treated and control states.

To tackle this concern, we follow Rambachan and Roth (2023), who provide conditions under which double-differences can be interpreted causally even in the presence of violations of parallel trends as long as post-trends do not deviate "too much" from a linear extrapolation of the pre-trends.[16] Rambachan and Roth (2023) formalize this idea with a sensitivity analysis that assesses that the slope of the pre-trends does not change by more than an arbitrary value M.[17] Figure 5 reports the results of this sensitivity analysis for our estimates. We compare the 95% confidence intervals obtained from our difference-in-differences estimation against those obtained after allowing for per-period deviations from a linear trend of up to $0 \leq M \leq 0.05$ for the first four post-treatment periods individually, as well as the average causal effect across six ($1 \leq \tau \leq 6$) and five ($2 \leq \tau \leq 6$) post-treatment periods. In red, we plot the original confidence intervals for β_5 from equation (2); in blue, we plot the fixed length confidence intervals (FLCIs) for different values of M. $M = 0$ corresponds to allowing only linear violations of parallel trends. Larger values of M allow for non-linear deviations. In the analysis of Panel A, for example, the FLCIs are similar to those from our baseline specifications when considering potential linear violations of parallel trends ($M = 0$) but become wider as we allow for more nonlinearity. The breakdown value for a significant effect is $M > 0.05$.[18]

B.2 Alternative Interpretations and Falsification Tests

To further support our interpretation of the AA scandal as a shock to trust in big business practices, we consider two alternative interpretations. First, general trends

[16] Other recently developed methodological papers that consider various relaxations of the parallel-trends assumption include Manski and Pepper (2018), Freyaldenhoven, Hansen, and Shapiro (2019), Keele, Small, Hsu, and Fogarty (2019), and Ye, Keele, Hasegawa, and Small (2020), among others. See also Roth (2022) for important limitations of testing for pre-existing differences in trends.

[17] For other empirical works that adjust for the extrapolation of a linear trend from the pre-treatment periods to alleviate the concern about violations of the parallel trends assumption, see Bhuller, Havnes, Leuven, and Mogstad (2013); Dobkin, Finkelstein, Kluender, and Notowidigdo (2018); Goodman-Bacon (2018); Goodman-Bacon (2021), among others.

[18] For similar applications of Rambachan and Roth (2023) in other laboratories, see Ang (2021), Miller, Johnson, and Wherry (2021), Alpert, Evans, Lieber, and Powell (2022), Bailey, Sun, and Timpe (2022), and Dustmann, Lindner, Schönberg, Umkehrer, and vom Berge (2022).

in the consulting industry might drive the patterns in columns (1)-(3) of Table 3; for instance, local firms might start choosing small consulting shops over large consulting firms after 2002, and the contracts signed with these two types of consultants might differ systematically. Or, trust in all large consulting companies might have dropped more than trust in small shops after the AA scandal. Table A.3 in the Online Appendix shows that the sum of the market shares of Big 5 firms is well below 1 in most US states, which suggests that systematic differences in the behavior of Big 5 and small consultants have the scope to drive our results.

To address this concern, we re-estimate equation (1) using dummies for the other Big 5 consulting companies. If our results were capturing general trends in consulting over time, such as changing contract boilerplates by large consulting companies, we should find similar estimates as those discussed above. Instead, in columns (4)-(7) of Table 3 we fail to reject the null hypothesis that trust in big business practices did not change across states based on the 2000 share of clients of non-AA Big 5 consultants. The coefficients are, if anything, positive, although economically small and statistically insignificant.

A second alternative explanation is that the Enron scandal might have also affected trust in *other* institutions that are important for contract design. For instance, trust in the judicial system, which is crucial to the enforcement and hence the design of contract. We exploit the fact that the Gallup Trust Survey elicits trust for a broad set of institutions at the same time and on the same respondents. In columns (8)-(10) of Table 3, we estimate equation (1) replacing the outcome variable with the trust in other institutions: small business practices, the judicial system, and banks. We find no economically or statistically significant changes after 2002 across states with a higher or lower share of AA clients in 2000.[19]

Overall, the falsification tests suggest that the drop in trust in big business practices predicted by the pre-scandal share of AA clients across US states is peculiar to AA rather than other large consulting firms and does not percolate to other institutions that are

[19]In untabulated results, we also repeat this exercise for the other institutions the Gallup Trust Survey considers, and we find similar non-effects across the board. We do not tabulate these results because the connection between the other institutions, such as the Armed Forces, and contract design is not obvious, but the results are available upon request.

relevant for contract design and enforcement.

V The Effect of Trust on Contracting

We move on to estimate the effect of a drop in trust between principals and agents on contract completeness. Note that our setting does not require that the small and often inattentive shareholders of public companies are aware of the details of negotiations with external consultants. Shareholders' trust is important because if a negative contingency is realized ex-post and the management had not planned for such contingency, the management might face distrustful shareholders' indemnification action. Managers thus have the incentive to complete contracts in environments in which shareholders' trust in agents is low irrespective of whether shareholders are aware of the contract negotiations when they happen.

A. Difference-in-Differences Analysis

We use a difference-in-differences strategy, which, unlike an instrumental-variable (IV) analysis, does not require us to make assumptions about the aggregation of individual-level trust information at the state level:[20]

$$Completeness_{i,j,s,p,t} = \alpha + \beta\ Treated_s \times After\ 2002_t + X'_{j,s,p,t}\delta + \eta_t + \eta_p + \eta_s + \epsilon_{i,j,s,p,t}, \quad (3)$$

where $Completeness_{i,j,s,p,t}$ measures the completeness of contract i signed by firm j in state s and industry p as of year t. In our baseline analysis, $Completeness_{i,j,s,p,t}$ is the number of topics contract i covers. *Treated* is a dummy variable that equals 1 if firm j is headquartered in a US state in which the share of local public firms audited by AA in 2000 was in the top 25% of the distribution, and zero otherwise, or the underlying continuous share of public firms in a state that were audited by AA in 2000. We add a full set of year fixed effects (η_t), industry fixed effects (η_p), and state fixed effects

[20] In a previous version of the paper, we had also added an IV analysis for robustness purposes that provided qualitatively similar results, but the instrument was weak. We do not describe those results for brevity but they are available upon request.

(η_s). In the most complete specifications we add a set of contract-, firm-, and state-level time-varying characteristics: *8K Reporting* indicates whether the contract was reported in an 8-K filing. *Company* indicates that the consultant is a legal person (rather than an individual). *Arbitration* equals 1 if arbitration is indicated to solve disputes. We also add the average return and volatility of the firm's stock at each end of the fiscal year. Finally, we control for the logarithm of states' yearly GDP to capture state-level business cycles. The sample period is between 1994 and 2015 and the sample includes all the contracts signed between a firm and outsider consultants. Because the outcome is a count variable, we use a negative binomial estimator. We cluster standard errors at the state level.

We report the baseline estimates of equation (3) in Table 5. The estimated coefficient attached to $Treated_s \times After\ 2002_t$ is positive, stable, and statistically different from zero across specifications (columns (1)-(3)), which reduces concerns that unobserved heterogeneity might explain the results. In columns (4)-(6), we estimate equation (3) using the continuous value of the 2000 state-level share of AA-audited public firms and the results are qualitatively similar.

In terms of economic magnitudes, the coefficients for $Treated_s \times After\ 2002_t$ show that, after 2002, the logarithm of the number of topics in contracts signed by firms at the top of the state-level distribution by pre-shock share of AA-audited firms is 0.101 higher than that in other contracts both before 2002 and across firms in states with a lower pre-shock share of AA-audited firms. The average of the outcome variable is 22.96 topics, whose natural logarithm is 3.13, and $3.13 + 0.101 = 3.231$, whose level corresponds to 25.30. Thus, a contract signed in a state with a high share of AA-audited public firms in 2000 covers 2.34 topics more after 2002 relative to before 2002 and to contracts signed in other states, which is about 10% more topics.

B. Parallel-Trends Assumption

To assess the plausibility of interpreting our double-differences estimates causally, we first estimate a version of equation (3) in which we include interactions between the treatment variable and each time-period dummy for the years before the AA scandal to assess the

dynamics of pre-trends and compare then with post-trends:

$$Completeness_{i,j,s,p,t} = \alpha + \sum_{\tau=t}^{T} \beta_\tau \times Treated_s \times Period_\tau + X'_{j,s,p,t}\delta + \eta_t + \eta_p + \eta_s + \epsilon_{i,j,s,p,t}, \quad (4)$$

where $Period_\tau$ is a dummy variable that indicates the τth period relative to the event year 2002.

We perform either the negative binomial regression that corresponds to the specification in Table 5 (see equation (3)) or the ordinary least squares (OLS) regression that corresponds to the specification for Panel A of Table 6.

We report the estimated coefficients in Table 7. Similar to the pre-trends assessment for trust in big business practices, we fail to detect any systematically different trends in contract completeness across states with a higher or lower share of locally listed companies that were audited by AA just before the scandal.

Figure A.3 of the Online Appendix presents the sensitivity analysis of our estimates to violations of the parallel-trends assumption. We compare 95% confidence intervals obtained from our baseline estimation against those obtained after allowing for per-period deviations from a linear trend of up to an arbitrary amount ($0 \leq M \leq 0.05$) with respect to the average causal effect across six $1 \leq \tau \leq 6$ and five $2 \leq \tau \leq 6$ post-treatment periods, respectively. We fail to reject the null of no effect on contract completeness during the post-treatment period of $1 \leq \tau \leq 6$ but we reject the null during the post-treatment period of $2 \leq \tau \leq 6$, which is consistent with the plausible possibility that the shock to trust started to transmit to contracting practices over time.

C. Robustness

In Table 6, we assess the robustness of the baseline result across alternative specifications and different measures. For each Panel, columns (1)-(3) use the dummy that equals 1 if the contract is signed in a state in the top 25% of the distribution of AA-audited public firms as of 2000, and zero otherwise as the main covariate of interest. In columns (4)-(6), we use the continuous value of the share of AA-audited public firms in states as of 2000.

In Panels A and B, we propose OLS specifications. As discussed above, linear

estimators are likely to be biased with count outcome variables, but nonetheless we confirm the robustness of our baseline estimates.

In Panel C, we weigh observations based on the number of firms in each state. These specifications put more weight on the contracts signed in large states—California, Texas, New York, Illinois, Massachusetts, and New Jersey.[21] Small states do not drive our results. If anything, the estimated magnitudes are slightly larger in the weighted specifications.

In Panels D and E, we use the two alternative topic-based measure of contract completeness based on 100 topics or 300 topics, and our results barely change. This test verifies that the assumption about the optimal number of topics in our LDA algorithm is not consequential to our results.

Finally, in Panels F, G, and H, we use the count measures of contract completeness we discussed in section III, that is, the number of total/unique words and sentences in each contract. Our baseline effect is replicated when we use count-based measures of contract completeness.

D. Falsification Tests

Our first falsification test to support our interpretation of the baseline results focuses consulting contracts signed with firm insiders. Trust should have barely any role in the design of contracts with insiders: if shareholders did not trust insiders/managers they would simply liquidate their holdings (Giannetti and Wang (2016)). By revealed preference, shareholders who stay invested must trust the management to run operations appropriately. Columns (1) and (6) of Table 8 show that contract completeness does not differ systematically for insider contracts. We interpret this test as another way to support our interpretation of the interaction *Treated* $\times$ *After 2002* as capturing a drop in trust as opposed to other contemporaneous shocks, which should have affected the design of all consulting contracts, including contracts with insiders.

We also use the state-level shares of local public firms audited by other *Big 5* auditing companies for a falsification test. Because these shares, as we showed earlier, do not predict a drop in trust, we should find no effect on contract completeness either.

[21] See the distribution of contracts by states in Table A.2.

In Table 8, columns (2)-(5) and columns (7)-(10) broadly corroborate this conjecture although we do reject the null statistically for one of the coefficients.

E. Selection: Change in Contracted and Disclosed Relationships?

By construction, our sample includes principal-agent relationships that are contracted and disclosed by principals. Consequently, our results face a selection concern: the types of relationships that are contracted and disclosed may have changed around the time of the AA scandal. Specifically, we might observe higher completeness after the scandal because the relationships that already required more complete contracts continued to be contracted upon, whereas those that required less complete contracts before the scandal ceased to be contracted upon and/or stopped being disclosed.[22]

Although by construction we cannot design a direct test to determine whether this form of selection is relevant in our setting, we can directly test whether any observable characteristics of the principals, agents, and contractual relationships in our sample changed systematically around the AA scandal and across US states. Detecting systematic differences would inform us about the extent and nature of this selection issue, while detecting no systematic differences would mitigate concerns that this form of selection could influence our results.

We run these tests considering both the time-series raw-data evidence, which we compare to the patterns of trust and contract completeness plotted in Figure 1 of the Introduction, and the multivariate difference-in-differences specification defined in equation (3).

E.1 Characteristics of Disclosing Firms and Agents over Time

Starting with disclosing firms, we find that the average number of contracts disclosed per firm did not change around 2002 (see Figure A.4), providing direct evidence that there was no systematic change, on average, in the number of disclosed relationships

[22]We thank Justin Murfin and Luigi Guiso for raising this issue.

entering our sample around the AA scandal. Moreover, the characteristics of disclosing firms we observe—such as firm size, leverage, or the share of high-tech firms, which might necessitate more complex contracts due to their business activities—do not exhibit the same patterns we uncover for contract completeness (see Panel A of Figure 6).

Moving on to consultants in disclosed contracts, the characteristics we observe include whether consultants are individual contractors or employees of consulting firms, whether they are external or internal to the firm, and whether they are CEOs of their companies. In Panel B of Figure 6, we find that none of these characteristics changed systematically around 2002.

To rule out the possibility that the lack of systematic time-series variation in any of the firm- and consultant-specific characteristics we observe might mask heterogeneity across high- and low-AA states, Table 9 presents multivariate specifications based on equation (3). In columns (1)-(3), we analyze firm-level characteristics, while columns (4)-(6) focus on consultant-level characteristics. Consistent with the raw-data time-series evidence, we find no economically or statistically significant differences in the characteristics of disclosing firms and consultants involved in disclosed contracts around the AA scandal and across high- and low-AA states.

E.2 Relationships Unlikely to be Contracted Upon

We also consider proxies for the types of contractual relationships that are pursued and codified in contracts before and after the AA scandal. Because of higher negotiation costs, after the AA scandal, firms might have dismissed a set of potential business opportunities that, due to their nature, do not allow for long negotiations before contracting. For instance, business opportunities that require swift action on the part of the principal, which might be typically contracted upon by shaking hands or through gentlemen's agreements, might have been avoided by the firms most exposed to the AA scandal.

For an empirical test, we consider the evolution of the share of disclosed contracts that are very short, because relationships for which contract negotiations are impractical might be more likely to be codified in very short contracts. In Panel A of Table 10, we repeat our baseline analysis when using a dummy variable for whether a disclosed contract

is among the shortest we observe. We find no evidence that short contracts were more or less likely to appear in our data after 2002 and/or in high-AA states.

E.3 Relational Contracts

We also ask if principals revised their beliefs about the length of contractual relationships after the AA scandal. If contractual relationships were expected to be shorter, relational contracts would have not been a substitute for (formal) contract completeness anymore (Baker et al. (2002); Macchiavello (2018)). Principals might have thus started to push agents to write more complete contracts irrespective of their trust in agents.

Panel B of Table 10 shows that our treatment condition does not explain the average duration of contracts as stated at the time of signing, which suggests that changing expectations about relationships' length is an unlikely explanation for our results.

E.4 Changing Legal Standards Due to Litigation

Finally, contracts in high-AA states might have incorporated new clauses after 2002 if the AA scandal induced more litigation in high-AA states and court decisions created new legal standards by which local public companies had to abide.[23]

To assess whether this channel might be relevant and explain our results, we collected data from the *Federal Judicial Center's* (FJC) Integrated Database on the type of lawsuits, their object, and the litigating parties. We analyse these data in Table 11. We find no evidence that the number of civil lawsuits involving contract violation changed differentially after 2002—the coefficients change sign across types of litigation and are economically small and statistically insignificant.

VI Which Clauses Are Added When Trust Drops?

As we discussed in Section III, higher contract complexity only implies higher completeness if the additional clauses in the contract refer to verifiable contingencies,

[23]Hayes et al. (2021) find that a lower level of trust in a given state is associated with more complaints/disputes between banks and their customers in that state.

but our text-analysis-based measures do not capture the verifiability of contract clauses. To further corroborate that contracts become more complete after the negative shock to trust we consider rather than merely more complex, in this section we propose a complementary analysis to assess which specific clauses are more likely to be added to contracts. Once we identify the specific clauses that are more often added to contracts, we can more concretely assess whether the additional clauses refer to foreseeable and verifiable future states of the world for which earlier contracts were not planning.[24]

Note that this analysis, although helpful to understand contract completeness, cannot be our main empirical test due to a set of shortcomings. First, this analysis does not let us summarize contract features in one single variable or estimate the magnitude of the changes to contract features over time and across states, which is what we did in the first part of the paper. Second, this analysis is subject to a multiple-hypothesis-testing problem: because we identify 200 topics through the LDA procedure, testing for whether any of 200 topics are added to contracts consists of a large set of multiple hypotheses that could reject the null for at least some topics mechanically even if no significant relationships existed in the data. To alleviate this issue, we verify that the topics that are more likely to appear in contracts based on the difference-in-differences specification correspond to the topics that are more likely to appear in contracts based on simple averages in the raw data. Moreover, we look for common semantic patterns and verify that the topics that get added to contracts do belong to a similar semantic group, i.e. restrictions and impositions to agents on the part of principals.

Keeping these shortcomings in mind, in Table 12 we report the difference-in-differences estimates for equation (3) when the outcome variable is a dummy that equals 1 if a topic appears in the contract, and zero otherwise, for each of the 200 topics the LDA procedure identifies. In the table, we only report the results for the topics for which the interaction coefficient is positive and statistically significant.

To assess these topics qualitatively, in Figure 7 we show the cloud representation of

[24] An alternative approach would identify trust-sensitive topics from the list of all the topics contracts cover and assess whether these trust-intensive topics are more likely to appear in contracts after the AA scandal. Because we could not determine objective criteria to identify trust-sensitive topics, we started from the topics that appear more in contracts so that the reader can assess whether such topics are likely to complete contracts or merely make contracts more complex.

the elements of each topic—the words that constitute the topic as well as the probabilities attached to each word (font size)—similar to the examples we discussed in Figure 3. Note that topics related to more sparse clouds include many words whose probability of being part of the topic is so small that the associated font would not be readable, which is why we simply do not report those words.

A common theme across topics that appear more often in consulting contracts signed after 2002 in high-AA states is the imposition of restrictions to agents, which limit the agents' action during and after the contractual relationship. All the topics are compatible with the possibility that, due to the negative shock to trust we consider, principals started to impose stricter requirements to agents and to plan explicitly in the contract for potential future states of the world in which agents could have taken advantage of principals.

The first topic refers to confidentiality and the secrecy of proprietary data that the agents would access during the contractual relationship (Panel A). This topic includes words referring to information ("information," "documents," "operations") and specifically to the state of the world in which information is disclosed by agents to third parties ("disclosure," "confidentiality," "damages," "liability").

Because an overwhelming majority of the contracts in our sample include at least a clause labeled "confidentiality," we require restrictions placed on sensitive information regarding both the contractor and consulting service to be present in a contract. This group represents 12% of the contracts in our sample.

The second topic details the conditions that might cause the termination of the consulting agreement (Panel B). This topic includes terms related to the termination of the contract ("termination," "expiration," "effect,") as well as several potential causes that could trigger termination, which are presumably attributed to the agent ("cause," "failure," "death," "disability," "felony," "misconduct," "duties," etc.).

The third topic (Panel C) refers to potential amendments and conditions for amending the contract ("amendment," "consideration," "term," "witness," "force") and the fourth topic (Panel D) to indemnification—compensation for losses (presumably) suffered by the principal—("indemnification," "indemnitee," "settlement," "liability," "litigation").

The fifth topic we detect as appearing more often is depicted in Panel E of Figure 7. We refer to this topic as covering restrictions to agents' use of equity compensation, because many of the terms that compose the topic seem to refer to detailed conditions under which agents can exercise the equity incentives they are paid as part of their consulting activity: "stock options," "restricted," "vesting," "terms," "exercise," "expiration," "conditions," and "contingent."

The last type of clauses that appear more often based on our diff-in-diffs estimates (see columns (6)-(8) of Table 12) are non-compete agreements.[25] We estimate the specification in equation (3) where the outcome variable is a dummy that equals 1 if any non-compete-related topic exists in a contract, and zero otherwise.

Non-compete agreements oblige the agent not to engage in competing activities with the principal during and/or after the end of the consulting contract up to an agreed period of time. Such clauses aim to protect the principal and avoid that the agent might exploit proprietary information such as clients' contacts and proprietary business information (e.g., trade secrets) to engage in activities in competition with the principal.

Non-compete clauses are especially interesting for our analysis because the enforceability of such clauses differs systematically across US states (see, e.g., Jeffers (2018), Garmaise (2011), and Starr (2019)). This test thus gives us an additional source of variation: if non-compete clauses were added to contracts because of an explicit negotiation between principals and agents, rather than just because of using more complex contract templates, they should only appear more frequently after 2002 in high-AA states in which they are enforceable and not in other high-AA states.

And, indeed, when comparing columns (7) and (8) of Table 12 we find that the likelihood that non-compete clauses are added to consulting contracts in high-AA states after 2002 is positive, large, and statistically different from zero, whereas we cannot reject the null that these clauses are not more likely to be added in high-AA states in which they cannot be enforced. In fact, the estimated coefficient is negative in the latter case albeit statistically not different from zero. Note that the overlap between high-AA states and

[25]We do not provide a single cloud representation of non-compete agreements, because here we are pooling together three different topics all of which refer to non-compete as an important word in the topic.

enforcing states is quite low—the correlation between the continuous share of AA-audited firms in 2000 and the enforceability index is 5.4%, whereas the correlation between the two dummy measures in the analysis (high AA and high enforceability) is 3.5%. In untabulated results, we also find that no differences arise when assessing the likelihood of the other clauses we discussed above, which are similarly likely to appear more in high-AA states after 2002 irrespective of the extent of enforceability of non-compete agreements.

Overall, our analysis of the specific topics and clauses points in one direction: Contracts signed after the AA shock include more provisions for potential ex-post verifiable states of the world in which agents might take advantage of principals, which completes contracts rather than merely increasing their complexity. At the same time, adding these clauses and their discussion to the contract ensures agents against potential accusations of wrongdoing after actions that agents deemed lawful.

Note also that the added clauses (confidentiality, non-compete agreements, etc.) do not seem to refer to states of the world that were unforeseeable by principals and agents at the time of signing the contracts, for instance due to technological or regulatory shocks, but rather to states that were probably deemed unlikely and not worth negotiating upon. That the managers of companies regulated by the SEC could not imagine the possibility of agents' unfair competition or of disclosure of proprietary information seems rather implausible.

VII Conclusions

We document that, after a drop of trust between principals and agents, the completeness of principal-agent contracts increases. We do so using a large and novel sample of US consulting contracts. This result arises across several proxies of completeness and is corroborated by a set of falsification tests. The effects of changing trust on contract completeness do not fade. Moreover, our qualitative analysis of individual clauses reveals that added clauses clarify states of the world related to potential wrongdoing by agents against principals, which both distrustful principals and agents might ask to negotiate and add to contracts.

Whether completing contracts is triggered by shareholders' explicit requests to managers, by managers' intent to insure themselves against shareholders' actions if damages caused by agents verify—actions that were foreseeable before and after the shock—and/or by distrustful agents who want to clarify which actions could be deemed in violation of the contract are three non-mutually exclusive channels that should be investigated by follow-up research.

So far, the lack of data and methods of analysis of contractual characteristics has limited the empirical assessments of the predictions of the large body of theoretical literature in contract theory. Our paper contributes by providing empirical measures of contract completeness based on textual analysis techniques that can be used to investigate many other questions in empirical contract theory.

Our results also open paths for future research. Understanding the dynamics of the effects of trust on contracting—e.g., whether trust evolves and builds through continued interactions between parties—is an open question. Moreover, our results leave open the question of what is the right amount of trust (Butler et al. (2016)), that is, whether the higher contract completeness deriving from lower trust is ultimately welfare improving or reducing. More complete contracts impose higher negotiation costs on both parties and restrict agents' actions. At the same time, they might be beneficial to both parties as well as to other firm's stakeholders.

Also, our results focus on a setting in which one agent contracts with one principal. What is the theoretical and empirical effect of trust on contract design in multi-party settings, such as the case of syndicated loan contracts, in which each party is endowed with an amount of trust toward others? Do high and low levels of trust toward a counterpart transfer to others, for instance between a supplier and a customer engaging with the same party? Future research should study viable empirical and theoretical settings to answer these open questions.

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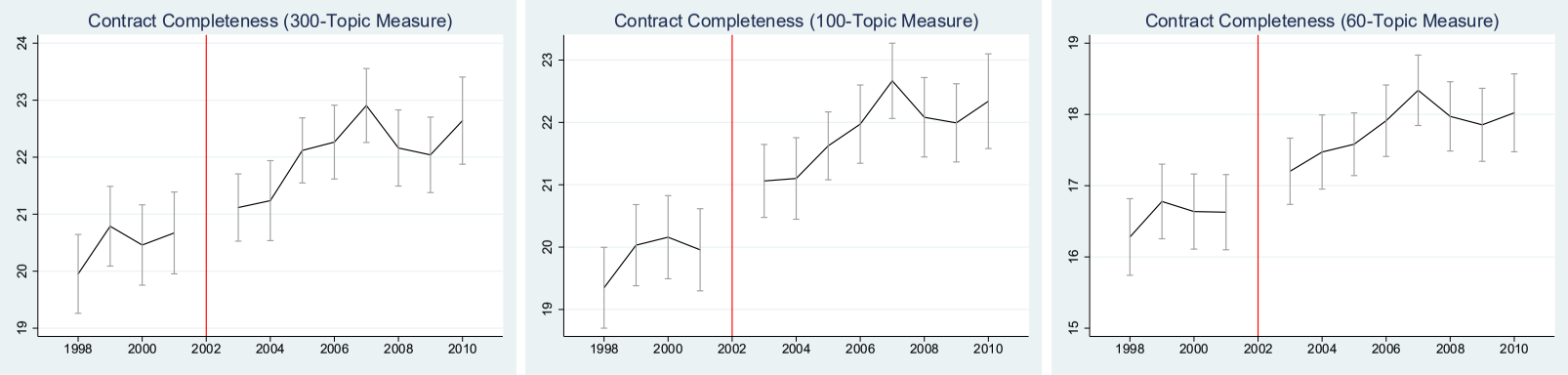
Figure 2: **Contract Completeness over Time: Other Measures**

In this figure, each panel reports the raw-data averages of one of the robustness text-analysis-based measures of contract completeness we construct in the paper. In each panel, the y-axis reports the average value of the measure reported on top of the graph in our sample of contracts for the year indicated on the x-axis.

A. Alternative Topic-Based Measures of Contract Completeness



B. Words and Sentences in Contracts

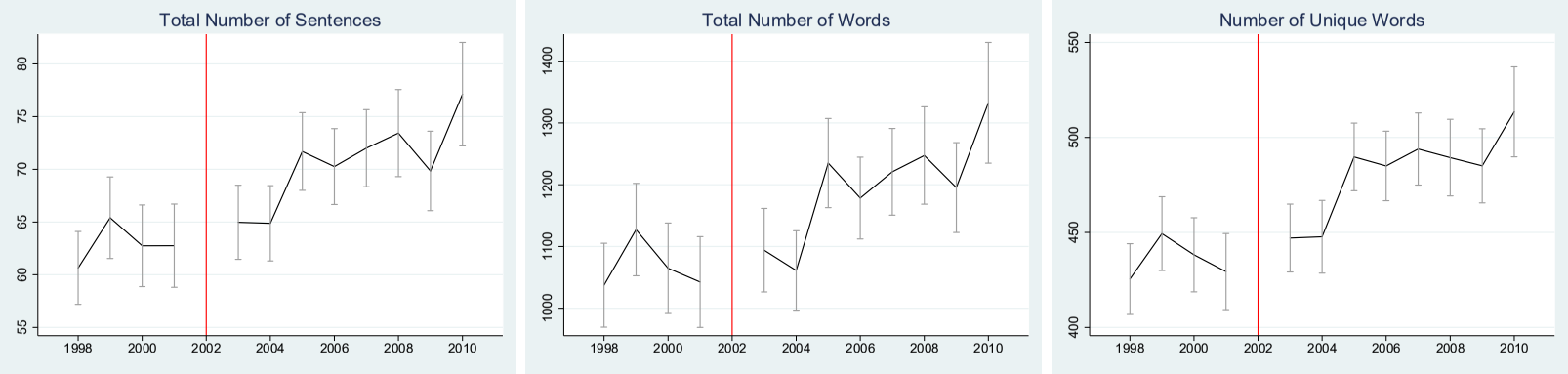
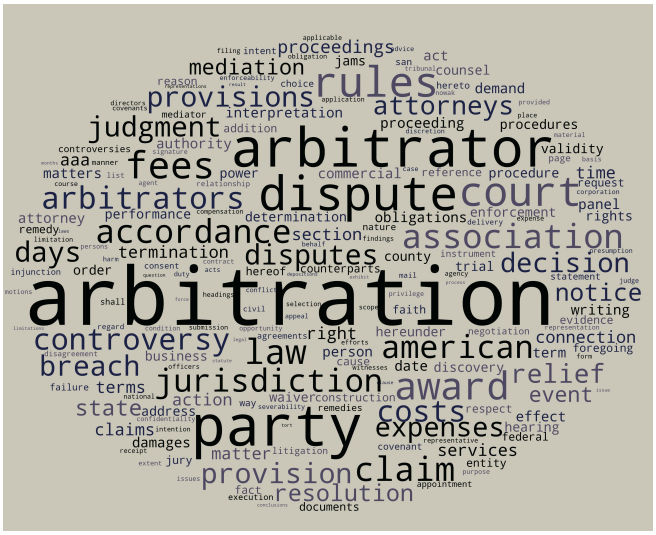


Figure 3: **Examples of LDA Topics Identified in Consulting Contracts**

This figure reports the cloud representation of the LDA-based matrix of terms and probabilities underlying the two examples of topics from our corpus of contracts that we discuss in Section III.

A. Arbitration to Solve Controversies Between Parties



B. Relationship to Company's Board Members

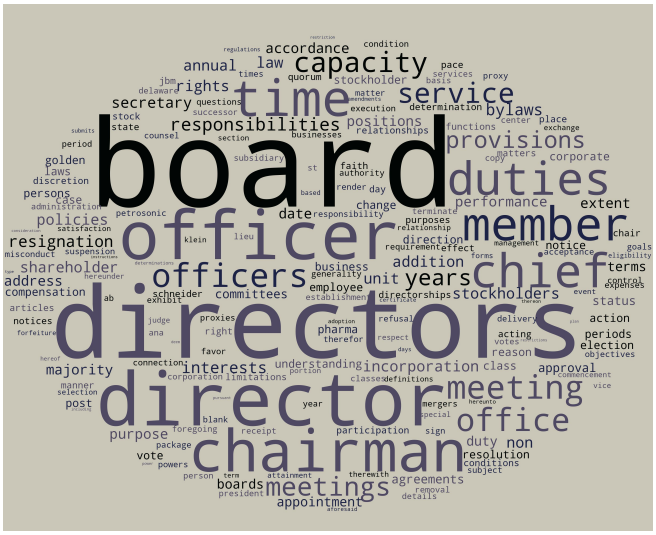


Figure 4: **Geographic Variation in the Share of AA Clients in 2000**

This figure is a state-level heatmap for the share of publicly listed firms in each state that were audited by AA in 2000. The darker is a state, the higher is the share. Values of the share are associated to colors based on the ranges in the figure's legend.

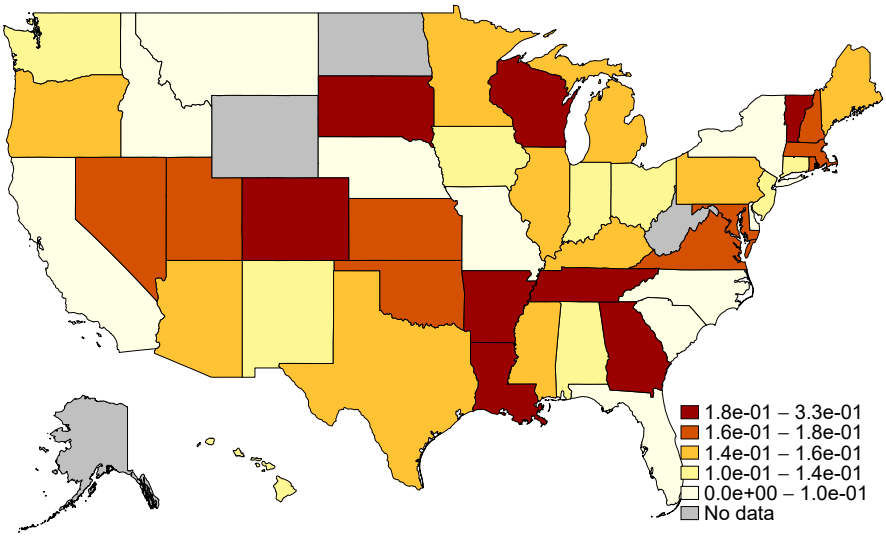


Figure 5: **Parallel-Trends Assumption: Sensitivity Analysis, Trust**

This figure shows sensitivity analysis of estimated effects on trust in big business practice to potential violations of the parallel trends assumptions per Rambachan and Roth (2023). The red bar in each panel represents the 95% confidence interval of the DiD estimate for relative time from estimation of equation (2). The blue bars represent corresponding 95% confidence intervals when allowing for per-period violations of parallel trends of up to $0 \leq M \leq 0.05$. That is, M represents the largest allowable change in the slope of an underlying linear trend between two consecutive periods.

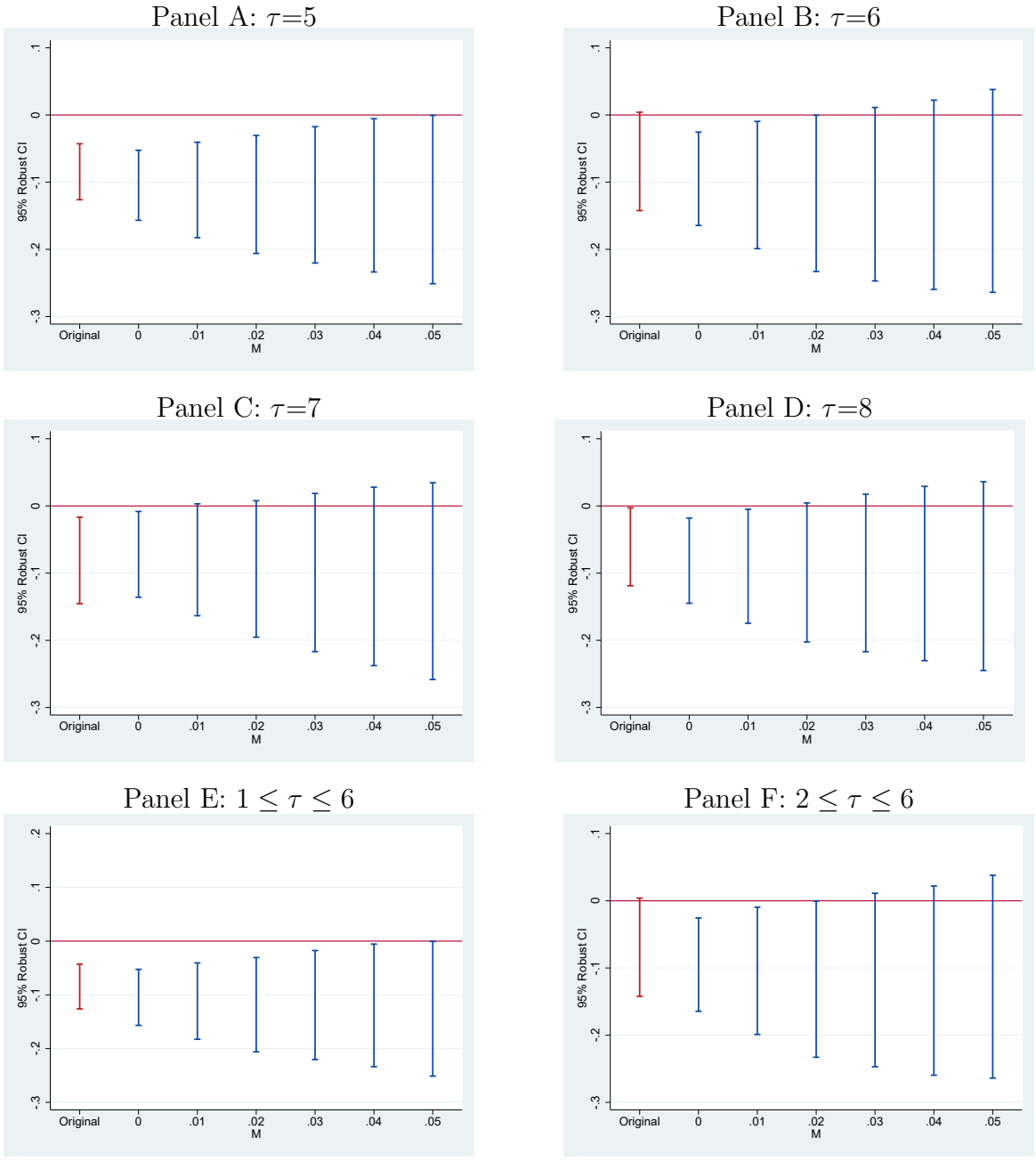


Figure 6: **Selection: Characteristics of Firms and Consultants Around AA Scandal**

In this figure, each panel reports the raw-data averages for the set of observables that capture the time-varying characteristics of firms (Panel A) and of consultants (Panel B) in our sample. In each panel, the y-axis reports the average value of the variable reported on top of the graph for the year indicated on the x-axis.

A. Contracting Firms by Observable Characteristics

B. Contracting Consultants by Observable Characteristics

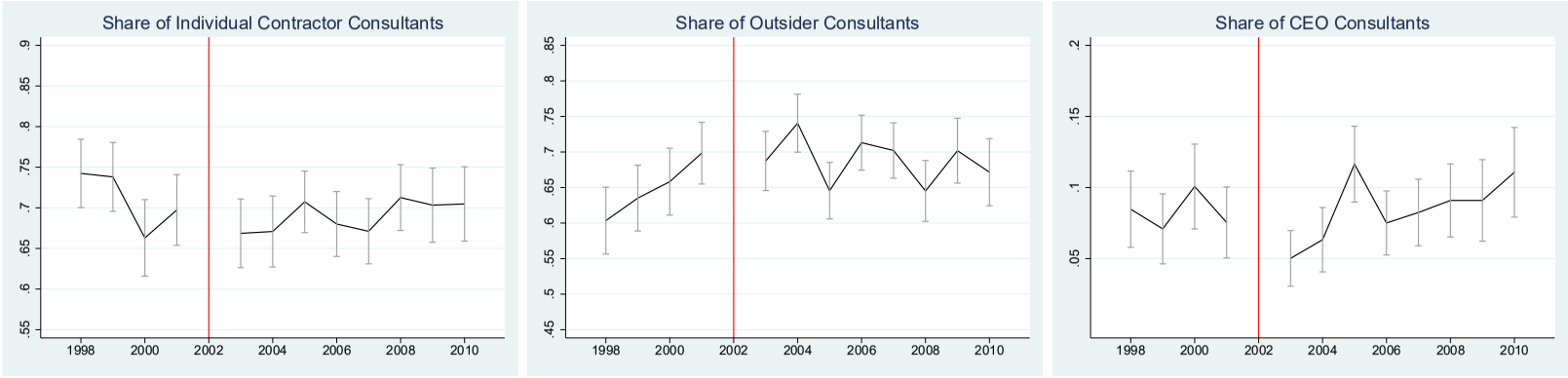


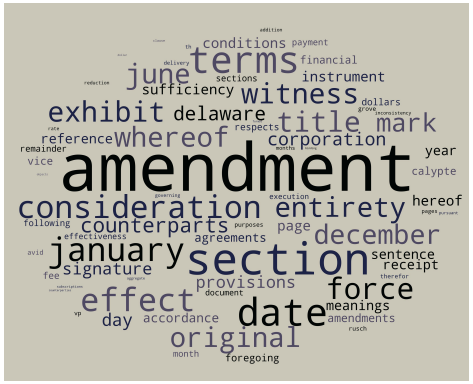
Figure 7: **Topics Appearing More Often in High-Exposure US States After the Negative Shock to Trust**

This figure reports the cloud representation of the LDA-based matrix of terms and probabilities underlying the topics that are most likely to appear in our sample's contracts after 2002 in high-AA-exposure states.

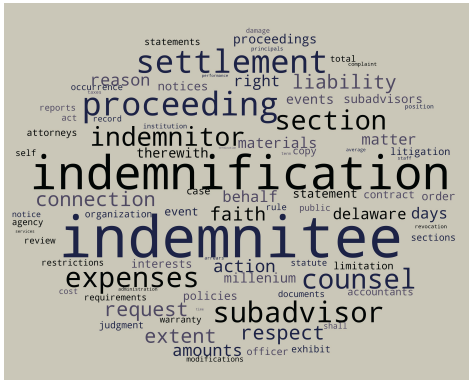
A. Confidentiality & Data Secrecy

B. Contract Termination

C. Contract Amendments



D. Indemnification



E. Restriction Equity to Agent

Table 1: **Examples of Client Firms and Consultants**

This table lists 30 examples that are randomly drawn from the sample of consulting agreements entered between publicly listed firms (clients) and external consultants.

Client	**Industry**	**State**	**Consultant**	**Company**	**Service**
Power Technology	Trading	AR	John Gray	No	n.a.
HESKA Corp	Pharmaceutical Products	CO	Robert B. Grieve	No	personnel
National Picture & Frame	Rubber and Plastic Products	MS	Harold G. Goodwin	No	n.a.
Alamo Energy Corp	Petroleum and Natural Gas	TX	Richard Edmonson	No	n.a.
Source Corp	Business Services	TX	David Lowenstein	No	n.a.
BIND Therapeutics, Inc.	Almost Nothing	MA	Gregory Berk	No	n.a.
Cablevision Systems	Communication	NY	John Tatta	No	personnel
AAIPharma	Business Services	NC	William L. Ginna, Jr.	No	n.a.
Golden Patriot	Petroleum and Natural Gas	NY	Sean Meegan	No	strategy, management finance, technology
Steve Madden	Apparel	NY	Charles Koppelman	No	n.a.
Servicemaster	Business Services	TN	Clayton, Dubilier Rice, Inc	Yes	n.a.
GK Intelligent Systems	Business Services	TX	Jon Pearman	No	finance, technology
Dreams	Retail	FL	Dreamstar, Inc.	Yes	n.a.
Ecologic Transportation	Personal Services	CA	Prominence Capital, LLC	Yes	finance
Mannatech	Pharmaceutical Products	TX	WonderEnterprises, LLC	Yes	strategy
Safety Components International	Textiles	NC	Francis X. Suozzi	No	personnel
ev3	Medical Equipment	MN	Matthew M. Jenusaitis	No	n.a.
Pacific Office Properties Trust	Trading	HI	Shidler Pacific Advisors LLC	Yes	n.a.
NTN Buzztime	Communication	CA	Jabam, Inc	Yes	n.a.
Marsh & McLennan Companies	Insurance	NY	Robert Clements	No	finance
American Realty Investors	Real Estate	TX	Prime Asset Management	Yes	finance
Liferate Systems	Business Services	MN	Jeffrey B. Comer	No	n.a.
NCT Group	Recreation	CT	Morton Salkind	No	n.a.
Netter Digital Entertainment	Entertainment	CA	Geoffrey Talbot	No	strategy, management finance, production
Sontra Medical	Medical Equipment	NJ	Dr. Robert S. Langer	No	technology
North American Technologies Group	Rubber and Plastic Products	TX	Slash Pine Ventures	Yes	accounting
Contango Oil & Gas	Petroleum and Natural Gas	TX	Juneau Exploration, LP	Yes	technology
SGI International	Machinery	CA	Elon A. Place	No	personnel
PICO Holdings	Real Estate	CA	Ronald Langley	No	n.a.
Carnegie Bancorp	Banking	NJ	Bruce A. Mahon	No	n.a.

Table 2: **Descriptive Statistics**

This table reports summary statistics for the main variables we use in the analysis. The sample unit is at the contract level. Post is a dummy variable that equals 1 if a consulting contract is signed after January 1, 2003, and zero otherwise. Outsider refers to contracts signed between companies and external consultants. Insider refers to contracts signed between companies and internal consultants. Public refers to contracts signed between publicly listed firms and any consultants. Private refers to contracts signed between private firms with SEC registration and any consultants. AA% is the fraction of public firms in a state that were audited by Author Andersen as of the end of fiscal year 2000. EY%, Deloitte%, PWC% and KPMG% are the fraction of public firms in a state that were audited by Ernst & Young, Deloitte, PricewaterhouseCooper and KPMG, respectively, as of the end of fiscal year 2000. Completeness 100, 200, and 300 refer to the number of topics of each contract calculated by the Latent Dirichlet Allocation (LDA) with 100, 200, and 300 as the total number of topics, respectively. # Words is the total number of words of each consulting contract. # Unique Words is the total number of unique words of each contract. # Sentences is the total number of sentences of each contract. Non-Compete is dummy variable that equals 1 if a non-compete clause is included in the contract, and zero otherwise. Duration is the time (in months) from the start to the end of the contract. Confidentiality is a dummy variable that equals 1 if both restrictions placed on contractor and business services are included in the contract, and zero otherwise. Termination is a dummy variable that equals 1 if a termination clause is included in the contract, and zero otherwise. Contract Amendment is a dummy variable that equals 1 if a termination clause is included in the contract, and zero otherwise. Indemnification is a dummy variable that equals 1 if an indemnification clause is included in the contract, and zero otherwise. Restrictions to Equity is a dummy variable that equals 1 if a clause that restricts equity-based compensation is included in the contract, and zero otherwise. Renegotiation is a dummy variable that equals 1 if the contract is amended after contracting, and zero otherwise. 8k is a dummy variable that equals 1 if contracting with a consultant is disclosed to investors as a specific corporate event, and zero otherwise. Company is a dummy variable that equals 1 if the agent is a consulting company, and zero if the agent is an individual consultant. Arbitration is a dummy variable that equals 1 if the contract allows the two parties to choose an arbitrator if disputes arise, and zero otherwise. Return is the state-level annualized stock return (weighed by firm capitalization) over the previous 12 months. Volatility is the state-level standard deviation of market-adjusted returns (weighed by firm capitalization) over the previous 52 weeks. Ln(GDP) is the logarithm of state-level Gross Domestic Product in the year in which a contract is signed, and zero otherwise.

	Panel A. Full Sample — Contract Types							
	Total	Mean	Std	Min	p25	p50	p75	Max
# Total	6,081	3.310	3.581	1	1	2	4	32
# Total, post=0	1,931	2.177	1.808	1	1	2	3	13
# Total, post=1	4,150	3.087	3.159	1	1	2	4	25
# Outsider	4,067	3.312	3.754	1	1	2	4	32
# post=0, Outsider	1,271	2.128	1.730	1	1	2	3	13
# post=1, Outsider	2,796	3.139	3.280	1	1	2	4	25
# Insider	2,014	1.946	1.722	1	1	1	2	14
# post=0, Insider	660	1.694	1.673	1	1	1	2	11
# post=1, Insider	1,354	1.811	1.514	1	1	1	2	13
# Public	4,671	2.776	2.813	1	1	2	3	24
# post=0, Public	2,736	2.623	2.634	1	1	2	3	18
# post=1, Public	1,935	2.234	1.984	1	1	2	3	13
# Private	1,410	3.417	3.001	1	1	2	5	16
# post=0, Private	1,071	3.088	2.321	1	1	2	5	10
# post=1, Private	339	2.510	2.437	1	1	1	3	11

	Panel B. Full Sample — Contract Characteristics							
	Total	Mean	Std	Min	p25	p50	p75	Max
AA%	6,081	0.133	0.043	0	0.092	0.139	0.160	0.333
EY%	6,081	0.144	0.044	0	0.110	0.150	0.182	0.235
Deloitte%	6,081	0.106	0.031	0	0.087	0.104	0.113	0.364
KPMG%	6,081	0.103	0.030	0	0.089	0.107	0.120	0.211
PWC%	6,081	0.149	0.044	0	0.120	0.141	0.189	0.385
Post	6,081	0.626	0.484	0	0	1	1	1
Public	6,081	0.768	0.422	0	1	1	1	1
Outsider	6,081	0.669	0.471	0	0	1	1	1
Completeness 200	5,909	22.960	8.050	3	18	22	28	71
Completeness 100	5,909	21.115	7.097	3	16	21	25	62
Completeness 300	5,909	21.501	7.472	2	17	21	26	76
# Words	5,909	1167.741	970.474	100	609	972	1448	19439
# Unique words	5,909	465.083	216.539	48	314	443	585	2038
# Sentences	5,909	68.537	50.921	3	40	59	84	1067
Confidentiality	6,081	0.112	0.316	0	0	0	0	1
Termination	6,081	0.674	0.469	0	0	1	1	1
Contract Amendment	6,081	0.843	0.364	0	1	1	1	1
Indemnification	6,081	0.104	0.306	0	0	0	0	1
Restrictions to Equity	6,081	0.714	0.452	0	0	1	1	1
Non-compete	6,081	0.299	0.458	0	0	0	1	1
Duration	4,949	20.785	20.237	0.200	11.100	12.167	24.333	240
Renegotiation	6,081	0.035	0.183	0	0	0	0	1
8K	6,081	0.318	0.466	0	0	0	1	1
Company	6,081	0.293	0.455	0	0	0	1	1
Arbitration	6,081	0.292	0.455	0	0	0	1	1
Return	6,081	0.132	0.237	-0.737	0.008	0.139	0.278	1.424
Volatility	6,081	0.050	0.020	0	0.036	0.044	0.060	0.164
Ln(GDP)	6,081	13.112	0.951	9.750	12.451	13.113	13.919	14.755

	Panel C. Outsider Contracts — Contract characteristics							
	Total	Mean	Std	Min	p25	p50	p75	Max
AA%	2,772	0.133	0.043	0	0.092	0.139	0.160	0.333
EY%	2,772	0.145	0.044	0	0.110	0.150	0.195	0.235
Deloitte%	2,772	0.104	0.030	0	0.087	0.104	0.113	0.364
KPMG%	2,772	0.104	0.029	0	0.089	0.107	0.120	0.211
PWC%	2,772	0.150	0.044	0	0.120	0.141	0.189	0.385
Post	2,772	0.569	0.495	0	0	1	1	1
Completeness 100	2,772	20.003	6.793	4	16	20	24	52
Completeness 200	2,772	21.779	7.668	3	17	21	26	71
Completeness 300	2,772	20.430	7.146	2	16	20	24	64
# Words	2,772	1029.922	809.220	100	545.5	870	1278	15267
# Unique words	2,772	428.082	202.277	48	289	411	537	2038
# Sentences	2,772	61.279	42.403	3	37	54	75	843
Confidentiality	2,772	0.116	0.320	0	0	0	0	1
Termination	2,772	0.671	0.470	0	0	1	1	1
Contract Amendment	2,772	0.857	0.350	0	1	1	1	1
Indemnification	2,772	0.097	0.296	0	0	0	0	1
Restrictions to Equity	2,772	0.712	0.453	0	0	1	1	1
Non-Compete	2,772	0.244	0.429	0	0	0	0	1
Duration	2,304	19.864	19.664	0.200	9.733	12.133	24.333	195.700
Renegotiation	2,772	0.036	0.186	0	0	0	0	1
8K	2,772	0.280	0.449	0	0	0	1	1
Company	2,772	0.350	0.477	0	0	0	1	1
Arbitration	2,772	0.136	0.343	0	0	0	0	1
Return	2,772	0.138	0.236	-0.737	0.017	0.143	0.287	1.424
Volatility	2,772	0.050	0.021	0	0.035	0.044	0.060	0.164
Ln(GDP)	2,772	13.118	0.947	9.750	12.480	13.113	13.910	14.755

	Panel D. State-Year sample — Trust Survey							
	Total	Mean	Std	Min	p25	p50	p75	Max
# Respondents	1,193	22.966	21.887	1.000	8.000	17.000	29.000	158
Trust (Big Business)	1,193	0.213	0.142	0.000	0.132	0.200	0.281	1
Trust (Small Business)	1,193	0.282	0.342	0.000	0.000	0.000	0.636	1
Trust (Banks)	1,193	0.384	0.193	0.000	0.250	0.375	0.500	1
Trust (Judical System)	1,045	0.236	0.153	0.000	0.143	0.227	0.308	1
Post	1,193	0.535	0.499	0.000	0.000	1	1	1
AA%	1,169	0.148	0.070	0.000	0.106	0.154	0.181	0.333
EY%	1,169	0.125	0.063	0.000	0.102	0.140	0.164	0.235
Deloitte%	1,169	0.097	0.065	0.000	0.061	0.097	0.122	0.364
KPMG%	1,169	0.096	0.054	0.000	0.063	0.097	0.127	0.211
PWC%	1,169	0.134	0.074	0.000	0.091	0.129	0.167	0.385
White	1,193	0.839	0.148	0.000	0.765	0.857	0.950	1
Black	1,193	0.073	0.109	0.000	0.000	0.038	0.107	1
Republican	1,193	0.440	0.185	0.000	0.333	0.433	0.533	1
Protestant/Jewish	1,193	0.347	0.325	0.000	0.000	0.375	0.625	1
Male	1,193	0.496	0.164	0.000	0.412	0.500	0.571	1
Ln(age)	1,193	3.828	0.160	2.890	3.726	3.831	3.936	4.330
Married	1,193	0.303	0.299	0.000	0.000	0.333	0.559	1
High Income	1,190	0.619	0.181	0.000	0.500	0.625	0.727	1

Table 3: **Exposure to Arthur Andersen (AA) Scandal and Trust in Big Business Practices**

This table reports estimates of β from the following linear specification by weighted least squares:

$$Trust_{s,t} = \alpha + \beta \times Treated_s \times After\ 2002_t + X'_{s,t} \times \gamma + \eta_t + \eta_s + \epsilon_{s,t},$$

where $Trust_{s,t}$ is the share of respondents who report a high degree of trust in state s in year t in big business practices (columns (1)-(7)), small business practices (column (8)), judicial system (column (9)), and in banks (column (10)); in columns (1)-(3) and (8)-(10), $Treated_s$ is a dummy that equals 1 if state s is in the top 25% of US states based on the share of local public firms audited by Arthur Andersen in 2000; in columns (4)-(7), $Treated_s$ is a dummy that equals 1 if state s is in the top 25% of US states based on the share of local public firms audited by Ernst & Young, Deloitte, KPMG, and PWC in 2000, respectively; $After\ 2002_t$ is an indicator variable that equals 1 for the period 2003-2015, and zero for the period 1994-2002; $X_{s,t}$ is a vector of averaged respondent-level characteristics including the logarithm of age, race dummies, a dummy for whether the respondent identifies as a Republican voter, a dummy for whether the respondent is Protestant or Jewish, a dummy for male respondents, married respondents, and respondents in the top bracket of income (see Table 2 for details). η_s and η_t are full sets of fixed effects for states and years. We weigh observations by the number of respondents in a state. We cluster standard errors at the state level.

	Top 25% Share AA in 2000			**Top 25% Share Big 5 in 2000**				**Trust in Other Institutions**		
				EY	Deloitte	KPMG	PWC	Small Businesses	Judicial System	Banks
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treated × After 2002	-0.027***	-0.026***	-0.029***	0.009	0.008	0.009	0.025**	0.014	-0.030	0.008
	(0.008)	(0.009)	(0.009)	(0.030)	(0.013)	(0.010)	(0.011)	(0.009)	(0.021)	(0.013)
Treated	0.000									
	(0.009)									
Year FE	X	X	X	X	X	X	X	X	X	X
State FE		X	X	X	X	X	X	X	X	X
Individual Controls			X	X	X	X	X	X	X	X
N	1,168	1,168	1,165	1,165	1,165	1,165	1,165	1,165	1,165	1,021
Adj R-sq	0.171	0.186	0.226	0.222	0.224	0.220	0.227	0.478	0.521	0.211

Table 4: **Parallel Pre-Trends: Exposure to AA Scandal and Trust**

This table reports the estimated coefficients ($\hat{\beta}_\tau$) on interactions of the treatment variable Treated with time-period fixed effects from the following weighted-least-squares specification (year 2002 is the omitted category):

$$Trust_{s,t} = \alpha + \sum_{\tau=t}^{T} \beta_\tau \times Treated_s \times Period_\tau + X'_{s,t}\delta + \eta_t + \eta_s + \epsilon_{s,t},$$

where $Period_\tau$ is a dummy variable that indicates the τth period listed in the left column; $Trust_{s,t}$ is the share of respondents who report a high degree of trust in big business practices in state s in year t; $Treated_s$ is a dummy variable that equals 1 for states in which the share of local public firms audited by Arthur Andersen in 2000 was in the top 25% of the distribution, and zero otherwise; $X_{s,t}$ is a vector of averaged respondent-level characteristics including the logarithm of age, race dummies, a dummy for whether the respondent identifies as a Republican voter, a dummy for whether the respondent is Protestant or Jewish, a dummy for male respondents, married respondents, and respondents in the top bracket of income (see Table 2 for details). η_s and η_t are full sets of fixed effects for states and years. We weigh observations by the number of respondents in a state. We cluster standard errors at the state level.

	(1)
Treated × 1990-1995	-0.055
	(0.034)
Treated × 1996-1997	-0.016
	(0.049)
Treated × 1998-1999	-0.043
	(0.037)
Treated × 2000-2001	-0.044
	(0.033)
Treated × 2003-2004	-0.084***
	(0.021)
Treated × 2005-2006	-0.069*
	(0.037)
Treated × 2007-2008	-0.081**
	(0.033)
Treated × 2009-2010	-0.061**
	(0.030)
Treated × 2011-2012	-0.051
	(0.044)
Treated × 2013-2014	-0.071*
	(0.040)
Average Demographics	X
Year FE	X
State FE	X
N	1,116
Adj R-sq	0.226

Table 5: **Trust and Contracting**

This table reports estimates of β from estimating the following negative binomial regression:

$$Completeness_{i,j,s,p,t} = \alpha + \beta \times Treated_s \times After\ 2002_t + X'_{j,s,p,t}\delta + \eta_t + \eta_p + \eta_s + \epsilon_{i,j,s,p,t},$$

where $Completeness_{i,j,s,p,t}$ measures completeness of contract i signed by firm j in state s, industry p, and year t. Completeness refers to the number of topics of contract calculated by the Latent Dirichlet Allocation (LDA) with 200 as the total number of topics. In columns (1)-(3), Treated is a dummy variable that equals 1 if firm j is headquartered in a US state in which the share of local public firms audited by Arthur Andersen (AA) in 2000 was in the top 25% of the distribution, and zero otherwise. In columns (4)-(6), Treated is the underlying continuous share of public firms in a state that were audited by AA in 2000. After 2002_t is a dummy variable that equals 1 if the contract was signed in 2003 or later, and zero otherwise. Please refer to Table 2 for definitions of other variables. We cluster standard errors at the state level.

	Top 25% Share AA			**Share AA**		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.054**			-0.623***		
	(0.023)			(0.203)		
Treated × After 2002	0.107***	0.121***	0.101***	0.901***	0.792**	0.635**
	(0.041)	(0.035)	(0.030)	(0.330)	(0.332)	(0.270)
8K Reporting			0.023			0.021
			(0.016)			(0.017)
Company			0.124***			0.124***
			(0.018)			(0.018)
Arbitration			0.188***			0.188***
			(0.018)			(0.018)
Return			-0.050			−0.058
			(0.072)			(0.073)
Volatility			0.398			0.147
			(0.997)			(1.063)
Ln(GDP)			0.007			−0.019
			(0.131)			(0.120)
Constant	3.135***	3.113***	2.968*	3.105***	3.025***	3.232**
	(0.072)	(0.133)	(1.655)	(0.083)	(0.143)	(1.504)
Year FE	X	X	X	X	X	X
Industry FE		X	X		X	X
State FE		X	X		X	X
N	2,772	2,772	2,772	2,772	2,772	2,772
Pseudo R-sq	0.007	0.017	0.031	0.007	0.017	0.031

Table 6: **Trust and Contracting: Robustness**

This table provides various robustness check of the results in Table 5. In columns (1)-(3), Treated is a dummy variable that equals 1 if firm j is headquartered in a US state in which the share of local public firms audited by Arthur Andersen (AA) in 2000 was in the top 25% of the distribution, and zero otherwise. In columns (4)-(6), Treated is the underlying continuous share of public firms in a state that were audited by AA in 2000. Across different panels, dependent variables take a variety of alternative measures for contract completeness and unless otherwise noted we estimate negative binomial regressions. In Panel A, we perform OLS regressions and completeness is a dummy variable that equals 1 if Completeness200 is above the median of its sample distribution, and zero otherwise. In Panel B, we perform OLS regressions and completeness is measured as the number of topics of Completeness200 divided by 200. In Panel C, the dependent variable is Completeness200 and we perform weighted negative binominal regressions. We assign the number of public firms in each state s as of contracting year t as the weight. In Panels D and E, Completeness100 and Completeness300 are the dependent variables. In Panels F, G, and H, completeness is measured as the total number of words, sentences, and unique words of a contract, respectively. Please refer to Table 2 for definitions of other variables. We cluster standard errors at the state level.

	Top 25% Share AA			Share AA		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. OLS, Dummy Completeness 200 Above Median					
Treated × After 2002	0.186***	0.231***	0.207***	1.326***	1.497***	1.315***
	(0.032)	(0.037)	(0.039)	(0.366)	(0.358)	(0.356)
Adj R-sq	0.025	0.049	0.105	0.025	0.048	0.104
	Panel B. OLS, Completeness 200					
Treated × After 2002	0.012**	0.014***	0.012***	0.096**	0.091**	0.076**
	(0.005)	(0.004)	(0.004)	(0.037)	(0.039)	(0.032)
Adj R-sq	0.036	0.067	0.155	0.037	0.067	0.155
	Panel C. NB, W=# of firms by state-year, Topic 200					
Treated × After 2002	0.107***	0.131***	0.114***	0.698**	0.796***	0.678***
	(0.037)	(0.031)	(0.027)	(0.354)	(0.265)	(0.241)
Pseudo R-sq	0.008	0.016	0.031	0.008	0.016	0.031
	Panel D. NB, Completeness 100					
Treated × After 2002	0.117**	0.131**	0.115**	1.189***	1.043***	0.901***
	(0.051)	(0.053)	(0.045)	(0.339)	(0.370)	(0.305)
Pseudo R-sq	0.009	0.018	0.033	0.009	0.018	0.033
	Panel E. NB, Completeness 300					
Treated × After 2002	0.122***	0.140***	0.119***	1.007***	0.912**	0.765***
	(0.041)	(0.043)	(0.034)	(0.332)	(0.356)	(0.283)
Pseudo R-sq	0.006	0.016	0.030	0.006	0.016	0.030
	Panel F. NB, Total Number Words					
Treated × After 2002	0.248***	0.226***	0.165***	1.730**	1.409**	0.744*
	(0.065)	(0.053)	(0.054)	(0.775)	(0.640)	(0.447)
Pseudo R-sq	0.002	0.009	0.016	0.002	0.009	0.016
	Panel G. NB, Total Number Sentences					
Treated × After 2002	0.212***	0.207***	0.155***	1.731***	1.395**	0.876**
	(0.055)	(0.042)	(0.042)	(0.641)	(0.564)	(0.379)
Pseudo R-sq	0.004	0.015	0.027	0.004	0.015	0.027
	Panel H. NB, Total Number Unique Words					
Treated × After 2002	0.146***	0.159***	0.122***	1.087**	0.892**	0.456
	(0.035)	(0.037)	(0.039)	(0.468)	(0.438)	(0.336)
Pseudo R-sq	0.004	0.009	0.017	0.004	0.009	0.017
Year FE	X	X	X	X	X	X
Industry FE		X	X		X	X
State FE		X	X		X	X
Controls			X			X
N	2,772	2,772	2,772	2,772	2,772	2,772

Table 7: **Parallel Pre-Trends: Completeness and Shock to Trust**

This table reports the estimated coefficients ($\hat{\beta}_\tau$) on interactions of the treatment variable Treated with time-period fixed effects from the following negative binomial regression (year 2002 is the omitted category):

$$Completeness_{i,j,s,p,t} = \alpha + \sum_{\tau=t}^{T} \beta_\tau \times Treated_s \times Period_\tau + X'_{j,s,p,t}\delta + \eta_t + \eta_p + \eta_s + \epsilon_{i,j,s,p,t},$$

where $Period_\tau$ is a dummy variable that indicates the τth period listed in the left column; in column (1), Completeness is the number of topics of contract calculated by the Latent Dirichlet Allocation (LDA) with 200 as the total number of topics (Completeness200); in column (2), Completeness is a dummy variable that equals 1 if Completeness200 is above the median of its sample distribution, and zero otherwise. $Treated_s$ is a dummy variable that equals 1 for states in which the share of local public firms audited by Arthur Andersen in 2000 was in the top 25% of the distribution, and zero otherwise. η_p, η_s, and η_t are full sets of fixed effects for industries, states, and years. Please refer to Table 2 for definitions of other variables. We cluster standard errors at the state level.

	Negative Binomial	OLS
	(1)	(2)
Treated × 1990-1995	0.033	0.054
	(0.175)	(0.156)
Treated × 1996-1997	0.176	0.199
	(0.138)	(0.182)
Treated × 1998-1999	0.055	0.173
	(0.107)	(0.126)
Treated × 2000-2001	0.109	0.120
	(0.119)	(0.124)
Treated × 2003-2004	0.219**	0.246*
	(0.104)	(0.137)
Treated × 2005-2006	0.155***	0.401***
	(0.055)	(0.089)
Treated × 2007-2008	0.073	0.239*
	(0.103)	(0.131)
Treated × 2009-2010	0.292**	0.439***
	(0.114)	(0.162)
Treated × 2011-2012	0.252**	0.302**
	(0.107)	(0.131)
Treated × 2013-2014	0.319**	0.571***
	(0.128)	(0.163)
Controls	X	X
Year FE	X	X
State FE	X	X
Industry FE	X	X
N	2,745	2,745
Adj R-sq		0.086
Pseudo R-sq	0.031	

Table 8: **Trust and Contracting: Falsification Tests**

This table reports estimates of β from the following negative binomial regression:

$$Completeness_{i,j,s,p,t} = \alpha + \beta\ Treated_s \times After\ 2002_t + X'_{j,s,p,t}\delta + \eta_t + \eta_p + \eta_s + \epsilon_{i,j,s,p,t},$$

where $Completeness_{i,j,p,k,t}$ measures the completeness of contract i signed by firm j in state s and industry p as of year t. Completeness refers to the number of topics of contract calculated by the Latent Dirichlet Allocation (LDA) with 200 as the total number of topics. Treated is a dummy variable that equals 1 if firm j is headquartered in a US state in which the share of local public companies clients of Arthur Andersen (AA) in 2000 was in the top 25% of the distribution, and zero otherwise, in column (1). It is the underlying continuous share of public firms in a state that were clients of AA in 2000 in column (6). In columns (2)-(5) and (7)-(11), it is the same variable but computed for the other Big 5 consulting companies as indicated on top of each column. $After\ 2002_t$ is a dummy variable that equals 1 if the contract was signed in 2003 or later, and zero otherwise. In columns (6) and (12), we replace the outcome variable with the logarithm of the number of months of duration of the contract, which captures the length of the contractual relationship. Please refer to Table 2 for definitions of other variables. We cluster standard errors at the state level.

	Top 25% Share AA or Big 5					**Share AA or Big 5**				
	Insider	EY	Deloitte	KPMG	PWC	Insider	EY	Deloitte	KPMG	PWC
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treated ×	0.037	0.023	-0.109***	−0.058	0.003	0.060	0.409	−1.605***	0.500	0.389
After 2002	(0.049)	(0.028)	(0.037)	(0.047)	(0.028)	(0.391)	(0.330)	(0.466)	(0.534)	(0.408)
Year FE	X	X	X	X	X	X	X	X	X	X
Ind FE	X	X	X	X	X	X	X	X	X	X
State FE	X	X	X	X	X	X	X	X	X	X
Controls	X	X	X	X	X	X	X	X	X	X
N	1,766	2,772	2,772	2,772	2,772	1,766	2,772	2,772	2,772	2,772
Pseudo R-sq	0.037	0.031	0.031	0.031	0.031	0.037	0.031	0.031	0.031	0.031

Table 9: **Selection: Characteristics of Contracting Firms and Consultants Around AA Scandal**

This table reports estimates of β from the following linear specification:

$$Y_{j,s,t} = \alpha + \beta \times Treated_s \times After\ 2002_t + X'_{s,t} \times \gamma + \eta_t + \eta_p + \eta_s + \epsilon_{j,s,t},$$

where $Y_{j,s,t}$ can be a firm-level characteristic (size measures as the logarithm of employees for firm j in year t in column (1), long-term debt over assets for firm j in year t in column (2), intangible assets over total assets for firm j in year t in column (3)) or a consultant-level characteristic (a dummy variable indicating whether firm j in year t signed at least one contract with an individual consultants in column (4), a dummy variable indicating whether firm j in year t signed at least one contract with external consultants in column (5), and a dummy variable indicating whether firm j in year t signed at least one contract with internal consultants (column (6)). $Treated_s$ is a dummy that equals 1 if state s is in the top 25% of US states based on the share of local public firms audited by Arthur Andersen in 2000; $After\ 2002_t$ is an indicator variable that equals 1 for the period 2003-2015, and zero for the period 1994-2002. η_p, η_s, and η_t are full sets of fixed effects for industry, states, and years. Please refer to Table 2 for definitions of other variables. We cluster standard errors at the state level.

	Firm characteristics			**Consultant characteristics**		
	Ln(#Employees)	Leverage	Intangibility	Individual	Outsider	Insider
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × After 2002	0.075	0.008	0.005	-0.009	-0.009	-0.001
	(0.088)	(0.012)	(0.015)	(0.011)	(0.008)	(0.005)
Return	-0.220***	-0.003	-0.017**	-0.013	-0.008	-0.005
	(0.057)	(0.010)	(0.007)	(0.011)	(0.009)	(0.008)
Volatility	-4.079**	-0.563	-0.243	0.133	0.180	-0.076
	(1.604)	(0.381)	(0.218)	(0.249)	(0.159)	(0.204)
Ln(GDP)	0.356	-0.032	-0.046	-0.023	-0.010	0.024
	(0.316)	(0.047)	(0.073)	(0.023)	(0.017)	(0.017)
Constant	-4.489	0.864	0.887	0.369	0.175	-0.269
	(4.148)	(0.619)	(0.951)	(0.297)	(0.226)	(0.230)
Year FE	X	X	X	X	X	X
Ind FE	X	X	X	X	X	X
State FE	X	X	X	X	X	X
N	26,290	22,615	24,520	27,436	27,436	27,436
Ad. R-sq.	0.312	0.080	0.208	0.004	0.009	0.003

Table 10: **Selection: Types of Contractual Relationships Around AA Scandal**

This table reports estimates of β from estimating the following specification:

$$Alternative\ Outcome_{i,j,s,p,t} = \alpha + \beta \times Treated_s \times After\ 2002_t + X'_{j,s,p,t}\delta + \eta t + \eta_p + \eta s + \epsilon_{i,j,s,p,t},$$

where $Alternative\ Outcome_{i,j,p,k,t}$ is the length of the contract relationship (in months) in Panel A or a dummy variable that equals 1 if the contract includes at most 100 unique words in Panel B for contract i signed by firm j in state s and industry p as of year t. Treated is a dummy variable that equals 1 if firm j is headquartered in a US state in which the share of local public companies clients of Arthur Andersen (AA) in 2000 was in the top 25% of the distribution, and zero otherwise, in columns (1)-(3). It is the underlying continuous share of public firms in a state that were clients of AA in 2000. $After\ 2002_t$ is a dummy variable that equals 1 if the contract was signed in 2003 or later, and zero otherwise. Firm-level controls ($X'_{j,s,p,t}$) are the same as in Table 5. We cluster standard errors at the state level.

	Top 25% Share AA			**Share AA**		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. Very Short Contract (≤100 words)					
Treated	-0.106			-0.030		
	(0.153)			(0.019)		
Treated × After 2002	0.099	0.109	0.056	0.009	0.002	0.003
	(0.135)	(0.142)	(0.146)	(0.019)	(0.017)	(0.019)
N	2,772	2,772	2,772	2,772	2,772	2,772
Adj R-sq	0.01	0.03	0.04	0.01	0.03	0.04
	Panel B. Length of Contract Relationship (in months)					
Treated	-0.355			-0.064		
	(1.027)			(0.098)		
Treated × After 2002	0.365	1.048	0.719	0.129	0.123	0.116
	(1.172)	(1.090)	(1.077)	(0.139)	(0.137)	(0.139)
N	2,304	2,304	2,304	2,304	2,304	2,304
Adj R-sq	0.03	0.05	0.06	0.03	0.05	0.06
Year FE	X	X	X	X	X	X
Industry FE		X	X		X	X
State FE		X	X		X	X
Firm-level Controls		X	X		X	X

Table 11: **Selection: Litigation and Legal Standards Around AA Scandal**

This table reports estimates of β from estimating the following specification:

$$Number\ Lawsuits_{s,t} = \alpha + \beta\ Treated_s \times After\ 2002_t + X'_{s,t}\delta + \eta s + \eta_t + \epsilon_{s,t},$$

where Number Lawsuits$_{s,t}$ is the total number of civil lawsuits, the number of civil lawsuits involving local public firms, or the share of lawsuits involving public firms in state s and year t. We estimate the specification using a negative binomial estimator in columns (1)-(2) and (4)-(5) due to the count nature of the outcome variable. Treated is a dummy variable that equals 1 if firm j is headquartered in a US state in which the share of local public companies clients of Arthur Andersen (AA) in 2000 was in the top 25% of the distribution, and zero otherwise, in columns (1)-(3) and the continuous value of the share in columns (4)-(6). $X'_{s,t}$ includes the set of controls described in Table 2 averaged at the state-year level. We cluster standard errors at the level of state. The sample period is from 1994 to 2015.

	Top 25% Share AA			**Share AA**		
	All Civil Lawsuits	Public Firms Involved	Share Lawsuits w/ Public Firms	All Civil Lawsuits	Public Firms Involved	Share Lawsuits w/ Public Firms
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × After 2002	-0.028	−0.001	0.008	-0.422	−0.102	0.117
	(0.068)	(0.087)	(0.034)	(0.442)	(0.588)	(0.243)
Return	0.012	0.053	0.011	-0.003	0.041	0.014
	(0.034)	(0.049)	(0.016)	(0.034)	(0.050)	(0.016)
Volatility	-0.359	0.539	0.221	-0.430	0.645	0.342
	(1.035)	(1.263)	(0.312)	(1.047)	(1.265)	(0.295)
Ln(GDP)	0.268	−0.076	-0.121*	0.340	−0.029	−0.131∗
	(0.259)	(0.276)	(0.070)	(0.266)	(0.288)	(0.075)
Constant	-0.226	3.002	1.986**	1.372	5.124	2.090∗∗
	(2.729)	(2.912)	(0.852)	(3.097)	(3.353)	(0.921)
Year FE	X	X	X	X	X	X
State FE	X	X	X	X	X	X
N	1,271	1,271	1,271	1,271	1,271	1,271
(Pseudo) R-sq	0.27	0.23	0.65	0.27	0.23	0.66

Table 12: **Which Clauses Appeared More Frequently After the Shock to Trust?**

This table reports estimates of beta from the following OLS regression equation

$$Y_{i,j,s,p,t} = \alpha + \beta \ \times Treated_s \times After\ 2002_t + X' \times \gamma + \eta_t + \eta_p + \eta_s + \epsilon_{i,j,s,p,t},$$

In columns (1)-(5), $Y_{i,j,s,p,t}$ is a dummy variable for whether the topic identified by the LDA procedure listed on top of each column is detected in contract i signed by firm j in state s and industry p as of year t. The words that compose each topic are depicted in Figure 7. In columns (6)-(7), $Y_{i,j,s,p,t}$ is a dummy variable that equals 1 if any non-compete-related topic exists in a contract, and zero otherwise. Treated is the fraction of public firms in a state that were clients of Author Andersen as of the end of fiscal year 2000 (AA%). Please refer to Table 2 for definitions of other variables. We cluster standard errors at the level of state. The sample period is from 1994 to 2015.

	Confidentiality	Termination	Contract Amendments	Indemni-fication	Restrictions to Equity	Non-compete Clauses All	Enforcement High	Enforcement Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated × After 2002	0.309*	0.967**	0.570*	0.456*	1.005**	0.503	1.454***	−0.295
	(0.177)	(0.445)	(0.321)	(0.281)	(0.415)	(0.951)	(0.201)	(0.181)
Year FE	X	X	X	X	X	X	X	X
Industry FE	X	X	X	X	X	X	X	X
State FE	X	X	X	X	X	X	X	X
Controls	X	X	X	X	X	X	X	X
N	2,772	2,772	2,772	2,772	2,772	2,772	1,341	1,329
Adj R-sq	0.004	0.042	0.052	0.010	0.009	0.045	0.017	0.006

Online Appendix: Trust and Contracts: Empirical Evidence

Francesco D'Acunto, Jin Xie, and Jiaquan Yao

Not for Publication

Figure A.1: **Example of Consulting Contract in Our Sample**

EX-10.(HHHH) 10 dex10hhhh.htm CONSULTING AGREEMENT

Exhibit (10)(hhhh)

CONSULTING AGREEMENT

This CONSULTING AGREEMENT ("Agreement"), dated January 13, 2010, by and between The First American Corporation, a California corporation (the "Company"), and Frank V. McMahon ("Consultant"). The parties agree as follows:

1. Services. From the date hereof until November 30, 2011 (the "Term"), the Company has retained Consultant to provide, and Consultant agrees to provide, to the Company and its subsidiaries consulting services as reasonably requested by the Company (collectively, the "Services"), including, without limitation, those services as may be requested to transition employee, client, vendor and other relationships to employees of the Company or its subsidiaries and to complete transactions in which the Company or any of its subsidiaries are involved. Consultant shall report to the chairman of the board, the chief executive officer of the Company and their designees (each such individual a "Designated Representative").

2. Independent Consultant. Consultant is not an employee or agent of the Company for any purpose. Consultant is an independent Consultant, and he is not eligible to participate in or receive any benefit from any benefit plan, program or other arrangement that may from time to time be available to employees of the Company including, but not limited to, any health, disability, or life insurance, vacation or holiday pay, sick leave, profit sharing or pension plans. The Company will not provide workers' compensation coverage for Consultant. Consultant is solely responsible for payment of all applicable taxes and withholdings respecting all payments made under this Agreement, and for all claims, damages and/or lawsuits arising out of the acts of Consultant and Consultant's employees and agents. The Company shall prepare and file a Form 1099 with respect to the payments made to the Consultant hereunder. Consultant does not have authority to obligate or bind the Company in any way, and he will not attempt to do so. The Company shall reimburse Consultant only for those expenses he incurs in connection with performing the Services that are pre-approved in writing by an officer of the Company. The Company is interested only in the results obtained by Consultant, who shall have sole control of the manner and means of performing under this Agreement.

3. Compensation. In consideration for the Services to be rendered by the Consultant hereunder the Company shall pay Consultant the total sum of $1,058,388.00, payable

(a) $50,000 on May 30, 2010 and

(b) provided Consultant has not breached Section 7 of this Agreement:

(i) $479,194.00 on November 30, 2010 and

(ii) $44,099.50 per month on the 30th day of each month (or if not a business day, the immediately preceding business day) commencing December 30, 2010, with the final payment to be paid on November 30, 2011.

4. Company Property. All access to and use of Company Property must comply with the Company's policies and procedures, as defined by the Company from time to time.

Consultant agrees to vacate the Company's facilities (if and to the extent Consultant has been provided access thereto) and return all Company Property (if and to the extent Consultant has been provided such property) immediately upon termination of this Agreement for any reason, or sooner upon request by the Company, and Consultant will pay for any damage to Company Property resulting from Consultant's actions and omissions. Consultant will not use any Company Property for any purpose other than providing the Services, without the Company's express prior written consent. For purposes of this Agreement, "Company Property" is the facilities, equipment and other property provided to Consultant for access and/or use in connection with providing the Services.

5. Performance. Consultant agrees to provide the Services with due diligence in compliance with applicable laws and regulations, and in accordance with the highest professional standards of practice in the industry.

Consultant will report to and provide the Services in accordance with the instructions of the Designated Representative. The Company shall have no right to control Consultant in the method for performing the Services.

6. Non-Exclusivity of Services. Subject to Section 7, Consultant is free to pursue any and all outside activities and/or employment as Consultant desires, and Company acknowledges that Consultant will likely be involved in other business activities, contracting and/or employment.

7. Non-Compete and Non-Solicit. Section 6 of this Agreement notwithstanding, until November 30, 2010, Consultant will not, directly or indirectly, engage in or render any service of a business, commercial or professional nature to any other person, entity or organization, whether for compensation or otherwise, that is, or has indicated an intention to be, a Competitor (as defined below); provided, for the avoidance of doubt, that this Section 7 shall not preclude Consultant from being employed by or rendering services as an advisor to investment banking or private equity firms so long as in the course of such employment or the rendering of such services Consultant does not, directly or indirectly, engage in or render any services of a business, commercial or professional nature to any other person, entity or organization, whether for compensation or otherwise, that is, or has indicated an intention to be, a Competitor. In accordance with this restriction, but without limiting its terms, Consultant will not:

(a) be employed by, serve as a director to, consult with or provide advice to or otherwise participate in the operations of any Competitor;

(b) solicit customers, business, patronage or orders for, or sell any products or services for any Competitor;

(c) divert, entice, or take away, or attempt to divert, entice or take away, any customers, business, patronage or orders of the Company and its subsidiaries for the benefit of or on behalf of any Competitor; or

(d) promote or assist, financially or otherwise, any person, firm, association, partnership, corporation or other entity that is a Competitor.

The Company's sole remedy for a breach of this Section 7 shall be termination of the Company's obligation to make further payments of any amount pursuant to Section 3(b) and, for the avoidance of doubt, the Company shall not be entitled to other monetary damages or injunctive relief in the event of any such breach. For the avoidance of doubt, a breach of this Section 7 shall not (i) constitute a breach of that certain Separation Agreement and General Release, dated as of even date herewith, between the Company and Consultant (the "Separation Agreement"), except to the extent that the activity resulting in a breach of this Section 7 would constitute a breach of the Separation Agreement by its terms, (ii) shall have no effect on the vesting of the Bonus RSUs or the Other RSUs granted to Consultant in 2007 (each as defined in the Separation Agreement), except to the extent that the activity resulting in a breach of this Section 7 would constitute a breach of the RSU Agreements (as defined in the Separation Agreement) by their terms, (iii) shall have no effect on the vesting of the Initial RSA (as defined in the Separation Agreement) and (iv) shall have no effect on the exercisability of the Initial Option (as defined in the Separation Agreement)

For purposes of this Section 7, "Competitor" means a person or entity that is engaged in, or has indicated an intention to be engaged in, any of the businesses described in the section captioned "The Information Solutions Group" in Part I, Item 1 of the Company's Annual Report on Form 10-K for the year ended December 31, 2008 (including, without limitation, the subsections captioned as "Information and Outsourcing Solutions Segment", "Data and Analytic Solutions Segment" and "Risk Mitigation and Business Solutions Segment"), excluding amendments to that section, if any, filed after November 30, 2009. The foregoing notwithstanding, no person or entity shall be deemed a "Competitor" as a result of engaging in activities in which the Company was not actually engaged in as of November 30, 2009.

In the event any executive vice president or higher officer of the Company has determined that Consultant has breached this Section 7, the Company will notify McMahon of such breach within 10 business days thereof.

8. Scope of Restricted Activities. For the purposes of Section 7, but without limitation thereof, Consultant will be in violation thereof if Consultant engages in any or all of the activities set forth therein directly as an individual on Consultant's own account, or indirectly as a stockholder, partner, joint venturer, employee, agent, salesperson, consultant, officer and/or director of, or by virtue of the ownership by Consultant's spouse, child or parent of any equity interest in, any firm, association, partnership, corporation or other entity engaging in any or all of such activities; provided, however, Consultant's or Consultant's spouse's, child's or parent's ownership of less than one percent (1%) of the issued equity interest in any publicly traded corporation shall not alone constitute a violation of Section 7 of this Agreement.

9. Additional Covenants.

(a) Detrimental Activity. Until November 30, 2011, Consultant agrees to refrain from engaging in any Detrimental Activity (as defined below). For purposes of this Agreement, "Detrimental Activity" means at any time (i) using information received during employment with the Company and/or its affiliates or during the Term relating to the business affairs of the Company or any such affiliates in breach of an express or implied undertaking to keep such information confidential; (ii) directly or indirectly persuading or attempting to

persuade, by any means, any employee of the Company or any of its affiliates to breach any of the terms of his or her employment with Company or its affiliates; (iii) directly or indirectly making any statement that is, or could be, disparaging of the Company or any of its affiliates or any of their respective employees (except to the extent necessary to respond truthfully to any inquiry from applicable regulatory authorities or to provide information pursuant to legal process); (iv) directly or indirectly engaging in any illegal, unethical or otherwise wrongful activity that is, or could be, substantially injurious to the financial condition, reputation or goodwill of the Company or any of its affiliates; or (v) directly or indirectly engaging in an act of misconduct such as, embezzlement, fraud, dishonesty, nonpayment of any obligation owed to the Company or any of its affiliates, breach of fiduciary duty or disregard or violation of rules, policies or procedures of the Company or any of its affiliates, an unauthorized disclosure of any trade secret or confidential information of the Company or any of its affiliates or inducing any customer to breach a contract with the Company or any of its affiliates. For the avoidance of doubt, the Company and Consultant acknowledge and agree that competing with the Company and/or its affiliates, where such competition does not involve any of the activities described in the immediately preceding sentence of this Section 9(a), shall not constitute Detrimental Activity.

(b) Non-Solicitation. Until November 30, 2011, Consultant agrees to not directly or indirectly, disrupt, damage, impair or interfere with the Company's or any of its affiliates' business by raiding any of the Company's or such affiliates' employees or soliciting any of them to resign from their employment by the Company or any such affiliate.

10. Scope of Covenants. The Company and Consultant acknowledge that the time, scope, and other provisions of Sections 7, 8 and 9 have been specifically negotiated by sophisticated commercial parties and agree that they consider the restrictions and covenants contained in such Sections to be reasonable and necessary for the protection of the interests of the Company, but if any such restriction or covenant shall be held by any court of competent jurisdiction to be void but would be valid if deleted in part or reduced in application, such restriction or covenant shall apply with such deletion or modification as may be necessary to make it valid and enforceable. The restrictions and covenants contained in each provision of such Sections shall be construed as separate and individual restrictions and covenants and shall each be capable of being severed without prejudice to the other restrictions and covenants or to the remaining provisions of this Agreement.

11. Trade Secrets and Confidential Information. Consultant acknowledges and agrees that he has learned, obtained, acquired, and become aware of, and will learn, obtain, acquire and become aware of information about the Company, its affiliates and their businesses, including, without limitation, unique selling and servicing methods and business techniques, business strategies, financial information, training, service and business manuals, promotional materials, training courses and other training and instructional materials, vendor and product information, customer and prospective customer lists, other customer and prospective customer information, processes, inventions, patents, copyrights, trademarks and other intellectual property and intangible rights, legal matters, personal information regarding officers and other employees, and other business information (collectively referred to as "Confidential

Information"). Consultant specifically acknowledges that all such Confidential Information, whether reduced to writing, maintained on any form of electronic media, or maintained in the mind or memory of Consultant

and whether compiled by the Company or any of its affiliates or by Consultant derives independent economic value from not being readily known to or ascertainable by proper means by others who can obtain economic value from its disclosure or use, that reasonable efforts have been made by the Company and its affiliates to maintain the secrecy of such information, that such information is the sole property of the Company or an affiliate of the Company and that any retention and use of such information or rights by Consultant shall constitute a misappropriation of the Company's or its affiliates' trade secrets, rights or other property. Consultant agrees to refrain from disclosing any Confidential Information to any person, either orally or in writing, for any reason. Consultant acknowledges and agrees that any unauthorized disclosure of Confidential Information would cause irreparable harm to the Company and/or its affiliates (at such time or as of the date of this Agreement) and such conduct shall be subject to immediate injunctive relief.

12. Assignment. Consultant will not assign, transfer or subcontract any right in or obligation arising under this Agreement without the Company's prior written consent. Any assignment in violation of this paragraph shall be void. This Agreement is binding on and will inure to the benefit of each party's heirs, executors, legal representatives, successors and permitted assigns.

13. General. If any provision of this Agreement is deemed unenforceable, such provision shall be severed from this Agreement and the remaining provisions will remain in full force and effect. This Agreement is governed by and will be interpreted in accordance with the laws of the State of California, without regard to the conflicts of law provisions thereof, or of any other State. No modification of this Agreement will be binding upon either party unless made in writing and signed by a duly authorized representative of such party. The failure of the Company to require performance by Consultant of any provision hereof shall not affect the full right to require such performance at any time thereafter; nor shall the waiver by the Company of a breach of any provision hereof by Consultant be taken or held to be a waiver of the provision itself. This Agreement contains the entire agreement and understanding of the parties hereto with respect to the subject matter hereof, and mergers and supercedes all prior agreements, discussions and writings with respect thereto.

14. Termination. Consultant may terminate this Agreement at any time upon delivery of written notice to the Company. Upon delivery of such notice, Consultant's and the Company's obligations hereunder, shall terminate and be of no further force and effect; provided, however, that Sections 4, 9, 11, 12, 13 and 14 of this Agreement shall survive any such termination.

BY SIGNING BELOW, THE PARTIES ACKNOWLEDGE THAT THEY HAVE CAREFULLY READ AND UNDERSTAND THE OBLIGATIONS IMPOSED BY THIS AGREEMENT. NO PROMISES OR REPRESENTATIONS HAVE BEEN MADE BY THE PARTIES OTHER THAN AS EXPRESSLY SET FORTH IN THIS AGREEMENT.

IN WITNESS WHEREOF the undersigned have executed this Agreement as of the day and year first written above. The parties hereto agree that facsimile signatures shall be as effective as if originals.

THE FIRST AMERICAN CORPORATION

By: /s/ Kenneth D. DeGiorgio
Kenneth D. DeGiorgio
Senior Vice President

Dated: January 13, 2010

FRANK V. MCMAHON

/s/ Frank V. McMahon

Dated: January 13, 2010

Figure A.2: **Optimal Number of Topics under LDA**

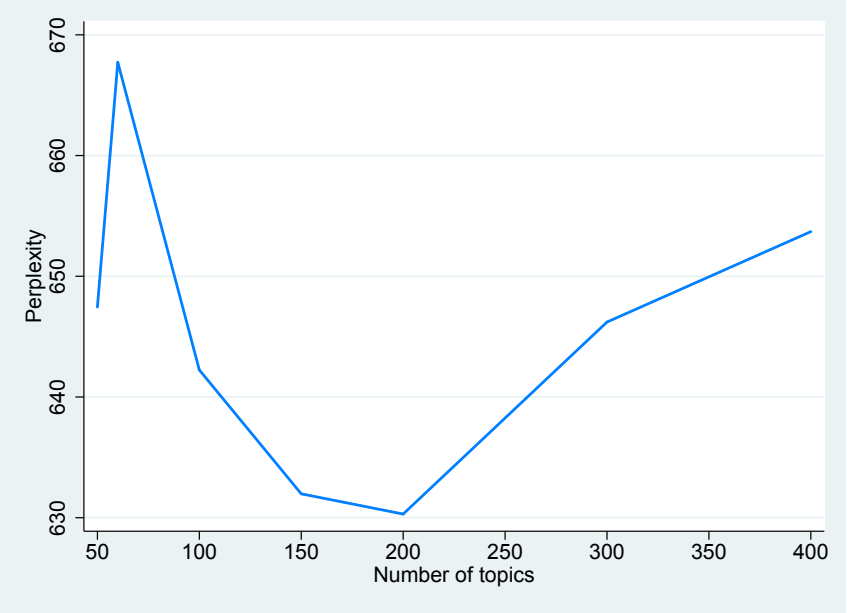
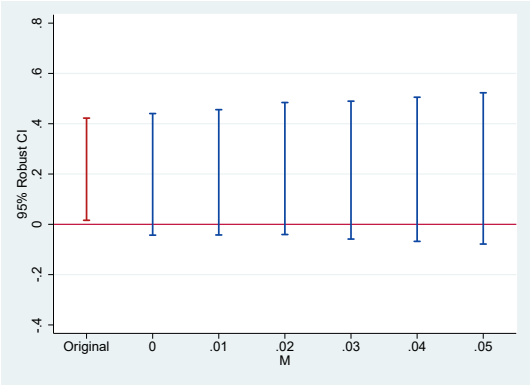


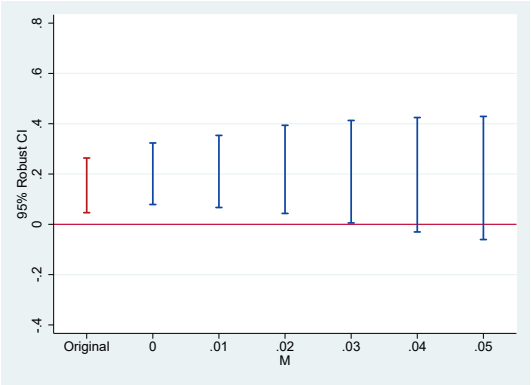
Figure A.3: **Sensitivity Analysis: Contract Completeness**

This figure shows sensitivity analysis of estimated effects on contract completeness to potential violations of the parallel trends assumptions per Rambachan and Roth (2023). The red bar in each panel represents the 95% confidence interval of the DiD estimate for relative time from estimation of equation (3). In Panels A and B, we perform negative binomial (NB) regressions and the dependent variable refers to the number of topics of contract calculated by the Latent Dirichlet Allocation (LDA) with 200 as the total number of topics (Completeness200). In Panels C and D, we perform ordinary least squares (OLS) regressions and the dependent variable is a dummy variable that equals 1 if Completeness200 is above the median of its sample distribution, and zero otherwise. The blue bars represent corresponding 95% confidence intervals when allowing for per-period violations of parallel trends of up to $0 \leq M \leq 0.05$. *That is, M represents the largest allowable change in the slope of an underlying linear trend between two consecutive periods.*

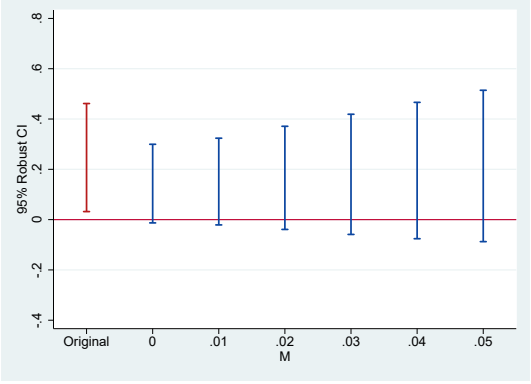
Panel A: $1 \leq \tau \leq 6$, NB



Panel B: $2 \leq \tau \leq 6$, NB



Panel C: $1 \leq \tau \leq 6$, OLS



Panel D: $2 \leq \tau \leq 6$, OLS

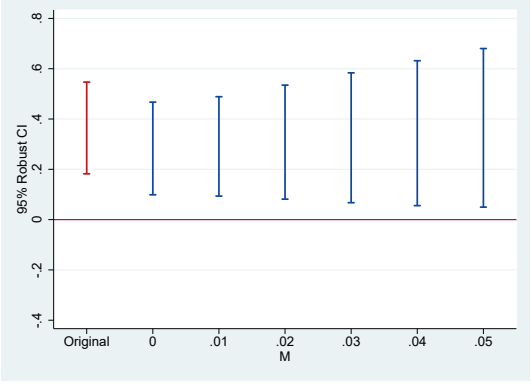


Figure A.4: **Trust or Changing Disclosure Requirements after SOX? Number of Contracts Disclosed Per Firm**

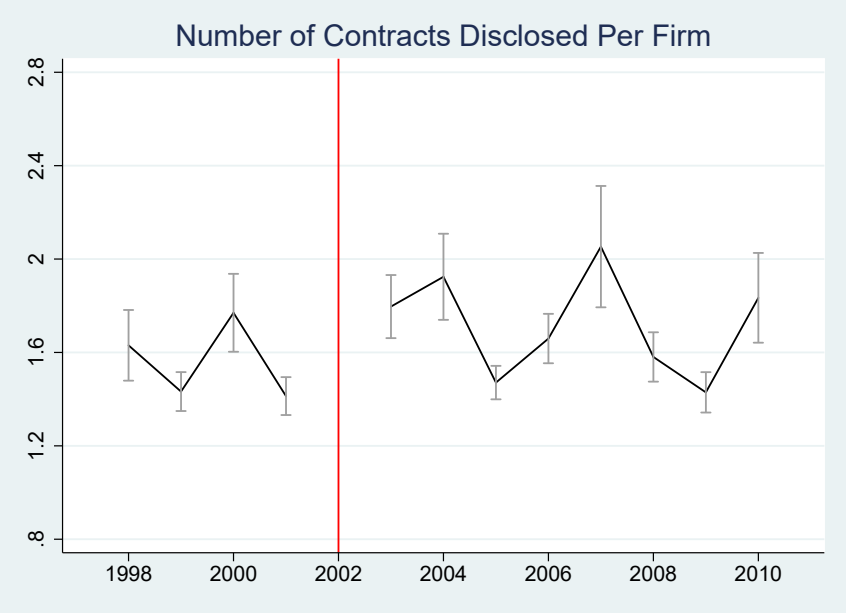


Table A.1: **Distribution of Consulting Contracts across Fama-French 49 Industries**

This table reports the distribution of consulting contracts across Fama-French 49 industries. Total refers to all contracts signed by companies in an industry. Outsider refers to contracts signed with external consultants. Insider refers to contracts signed with internal consultants. Public refers to contracts signed between publicly listed firms and any consultants. Private refers to contracts signed between private firms with SEC registration and any consultants. Min Yr and Max Yr refer to the earliest and latest years in which a contract is signed. 8K refers to contracts disclosed to investors as a specific corporate event.

	Total	Outsider	Insider	Public	Private	Min Yr	Max Yr	8-K
Agriculture	15	8	7	15	0	1995	2012	6
Food Products	64	40	24	56	8	1993	2015	21
Candy & Soda	16	10	6	11	5	1996	2014	5
Beer & Liquor	23	16	7	9	14	1994	2011	12
Tobacco Products	6	1	5	4	2	2002	2014	4
Recreation	43	32	11	30	13	1995	2015	6
Entertainment	161	120	41	119	42	1993	2015	48
Printing and Publishing	27	21	6	25	2	1996	2011	2
Consumer Goods	94	75	19	68	26	1992	2014	19
Apparel	42	24	18	35	7	1995	2015	13
Healthcare	116	83	33	102	14	1990	2015	30
Medical Equipment	216	149	67	175	41	1993	2015	76
Pharmaceutical Products	714	513	201	550	164	1995	2015	255
Chemicals	88	49	39	70	18	1993	2015	25
Rubber and Plastic Products	46	35	11	29	17	1995	2013	17
Textiles	30	19	11	19	11	1994	2009	4
Construction Materials	68	31	37	59	9	1994	2013	17
Construction	58	39	19	48	10	1995	2014	20
Steel Works Etc	64	26	38	53	11	1994	2015	26
Fabricated Products	14	3	11	13	1	1995	2015	4
Machinery	159	99	60	122	37	1991	2015	54
Electrical Equipment	99	76	23	78	21	1993	2014	40
Automobiles and Trucks	68	46	22	47	21	1993	2014	18
Aircraft	35	15	20	31	4	1996	2014	13
Shipbuilding, Railroad Equipment	14	8	6	8	6	1993	2013	2
Defense	13	7	6	11	2	1994	2011	0
Precious Metals	45	37	8	28	17	1993	2015	17
Non-Metallic and Industrial Metal Mining	101	83	18	40	61	1996	2014	59
Coal	17	12	5	12	5	1996	2014	7
Petroleum and Natural Gas	295	204	91	189	106	1993	2015	124
Utilities	133	59	74	117	16	1993	2015	25
Communication	172	118	54	110	62	1993	2015	59
Personal Services	93	43	50	79	14	1995	2014	15
Business Services	895	660	235	636	259	1994	2015	293
Computers	190	138	52	165	25	1994	2015	50
Electronic Equipment	227	155	72	193	34	1995	2015	56
Measuring and Control Equipment	95	66	29	74	21	1994	2015	20
Business Supplies	25	7	18	23	2	1994	2014	3
Shipping Containers	9	2	7	5	4	1997	2005	1
Transportation	105	55	50	90	15	1996	2014	20
Wholesale	154	106	48	112	42	1992	2015	65
Retail	219	140	79	165	54	1994	2015	62
Restaraunts, Hotels, Motels	79	45	34	71	8	1994	2015	18
Banking	268	115	153	235	33	1993	2015	94
Insurance	99	49	50	93	6	1995	2015	27
Real Estate	57	45	12	49	8	1993	2013	17
Trading	283	206	77	229	54	1994	2015	94
Almost Nothing	227	177	50	169	58	1993	2015	73

Table A.2: **Distribution of Consulting Contracts across States**

This table reports the distribution of consulting contracts across states. Total refers to the total number of contracts signed by companies in an industry. Outsider refers to contracts signed between companies and external consultants. Insider refers to contracts signed between companies and internal consultants. Public refers to contracts signed between publicly listed firms and consultants. Private refers to contracts signed between private firms with SEC registration and any consultants. Min Yr and Max Yr refer to the earlies and latest years in which a contract is signed. 8K refers to contracts disclosed to investors as a specific corporate event.

	Total	Outsider	Insider	Public	Private	Min Yr	Max Yr	8K
Alabama	21	12	9	21	0	1995	2014	4
Arizona	121	96	25	72	49	1988	2015	42
Arkansas	18	18	0	11	7	1999	2014	5
California	1195	819	376	922	273	1984	2015	368
Colorado	183	127	56	135	48	1994	2015	73
Connecticut	115	75	40	101	14	1989	2015	31
Delaware	15	8	7	12	3	1995	2015	5
District of Columbia	6	3	3	6	0	2001	2012	2
Florida	455	348	107	284	171	1992	2015	126
Georgia	189	131	58	155	34	1991	2015	61
Hawaii	16	9	7	12	4	1997	2015	6
Idaho	9	7	2	7	2	2001	2014	4
Illinois	210	126	84	166	44	1991	2015	61
Indiana	45	24	21	36	9	1995	2014	13
Iowa	13	7	6	13	0	1993	2014	2
Kansas	27	11	16	23	4	1996	2015	12
Kentucky	24	15	9	16	8	1994	2015	9
Louisiana	30	16	14	26	4	1995	2015	10
Maine	8	7	1	8	0	1995	2015	1
Maryland	82	53	29	73	9	1994	2014	31
Massachusetts	261	173	88	224	37	1981	2015	72
Michigan	93	51	42	76	17	1988	2014	26
Minnesota	107	73	34	100	7	1993	2015	22
Mississippi	11	8	3	8	3	1995	2013	1
Missouri	75	41	34	58	17	1992	2014	17
Montana	4	4	0	4	0	1996	2008	3
Nebraska	17	9	8	13	4	1997	2014	3
Nevada	202	175	27	99	103	1996	2015	80
New Hampshire	16	10	6	16	0	1995	2012	0
New Jersey	267	195	72	192	75	1990	2015	93
New Mexico	17	12	5	11	6	1995	2015	5
New York	541	366	175	415	126	1982	2015	177
North Carolina	121	68	53	99	22	1993	2014	41
North Dakota	1	0	1	1	0	2007	2015	0
Ohio	108	63	45	100	8	1985	2015	27
Oklahoma	25	9	16	22	3	1995	2013	5
Oregon	63	44	19	58	5	1995	2015	18
Pennsylvania	196	100	96	163	33	1993	2015	54
Rhode Island	27	22	5	9	18	1995	2009	8
South Carolina	47	43	4	26	21	1995	2014	15
South Dakota	8	7	1	7	1	2000	2010	3
Tennessee	55	22	33	53	2	1995	2014	12
Texas	657	421	236	522	135	1991	2015	262
Utah	73	49	24	58	15	1994	2015	28
Vermont	2	1	1	2	0	1999	2008	1
Virginia	123	78	45	109	14	1994	2014	43
Washington	113	90	23	63	50	1995	2014	31
West Virginia	6	2	4	4	2	2002	2015	2
Wisconsin	57	13	44	54	3	1994	2014	21
Wyoming	6	6	0	6	0	2005	2014	0

Table A.3: **Spatial Distribution of Big Five Clients across States as of 2000**

This table reports the distribution of big five's client shares of across states. AA% is the fraction of public firms in a state that were clients of Author Anderson as of the end of fiscal year 2000. EY%, Deloitte%, PWC% and KPMG% are the fraction of public firms in a state that were clients of Ernst & Young, Deloitte, PricewaterhouseCooper, and KPMG, respectively, as of the end of fiscal year 2000.

	AA%	EY%	Deloitte%	KPMG%	PWC%	N
Alaska	0.000	0.000	0.333	0.333	0.000	3
Alabama	0.246	0.197	0.115	0.049	0.213	61
Arkansas	0.212	0.242	0.121	0.091	0.030	33
Arizona	0.159	0.167	0.127	0.159	0.119	126
California	0.096	0.201	0.119	0.120	0.200	1574
Colorado	0.188	0.119	0.085	0.138	0.158	257
Connecticut	0.143	0.153	0.089	0.172	0.212	201
District of Columbia	0.333	0.133	0.033	0.200	0.033	29
Delaware	0.022	0.163	0.511	0.109	0.130	92
Florida	0.110	0.114	0.112	0.112	0.127	516
Georgia	0.261	0.150	0.129	0.108	0.108	286
Hawaii	0.105	0.211	0.105	0.316	0.105	19
Iowa	0.140	0.140	0.209	0.302	0.000	43
Idaho	0.063	0.000	0.188	0.063	0.313	16
Illinois	0.161	0.289	0.126	0.134	0.128	453
Indiana	0.107	0.191	0.122	0.061	0.160	131
Kansas	0.182	0.145	0.091	0.218	0.073	55
Kentucky	0.148	0.180	0.049	0.115	0.164	61
Louisiana	0.232	0.143	0.143	0.054	0.161	56
Massachusetts	0.186	0.115	0.140	0.105	0.272	511
Maryland	0.172	0.178	0.080	0.092	0.161	172
Maine	0.154	0.000	0.000	0.000	0.385	13
Michigan	0.151	0.139	0.163	0.036	0.163	166
Minnesota	0.147	0.228	0.095	0.129	0.125	231
Missouri	0.104	0.167	0.132	0.181	0.236	144
Mississippi	0.194	0.194	0.065	0.129	0.129	31
Montana	0.000	0.071	0.071	0.214	0.286	14
North Carolina	0.091	0.210	0.140	0.124	0.226	186
North Dakota	0.429	0.143	0.000	0.000	0.000	7
Nebraska	0.030	0.000	0.424	0.182	0.121	33
New Hampshire	0.176	0.147	0.176	0.118	0.118	33
New Jersey	0.122	0.118	0.150	0.122	0.127	428
New Mexico	0.111	0.000	0.000	0.167	0.056	18
Nevada	0.173	0.055	0.197	0.047	0.047	125
New York	0.096	0.141	0.117	0.127	0.202	957
Ohio	0.132	0.225	0.142	0.076	0.175	302
Oklahoma	0.194	0.222	0.097	0.111	0.069	72
Oregon	0.169	0.034	0.157	0.169	0.191	89
Pennsylvania	0.161	0.166	0.100	0.110	0.208	414
Rhode Island	0.207	0.207	0.069	0.276	0.069	29
South Carolina	0.034	0.119	0.169	0.220	0.085	59
South Dakota	0.333	0.000	0.000	0.000	0.167	12
Tennessee	0.209	0.235	0.113	0.096	0.174	115
Texas	0.164	0.179	0.118	0.148	0.151	865
Utah	0.167	0.147	0.059	0.069	0.088	100
Virginia	0.183	0.152	0.101	0.148	0.125	256
Vermont	0.467	0.067	0.067	0.067	0.067	15
Washington	0.139	0.145	0.164	0.115	0.194	165
Wisconsin	0.234	0.207	0.135	0.063	0.216	110
West Virginia	0.059	0.471	0.059	0.000	0.118	17
Wyoming	0.167	0.000	0.000	0.333	0.000	6