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Trust and Contracts: Empirical Evidence

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Abstract

Trust between parties should drive contract design: if parties were suspicious about each others' reaction to unplanned events, they might agree to pay higher costs of negotiation ex ante to complete contracts. Using a unique sample of U.S. consulting contracts and a negative shock to trust between shareholders/managers (principals) and consultants (agents) staggered across space and over time, we find that lower trust increases contract completeness. Not only the complexity but also the verifiable states of the world covered by contracts increase after trust drops. The results hold for several novel text-analysis-based measures of contract completeness and do not arise in falsification tests. At the clause level, we find that non-compete agreements, confidentiality, indemnification, and termination rules are the most likely clauses added to contracts after a negative shock to trust and these additions are not driven by new boilerplate contract templates. These clauses are those whose presence should be sensitive to the mutual trust between principals and agents.

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

Keywords: Empirical Contract Theory, Incomplete Contracts, Cultural Economics, Beliefs and Choice, Personnel Economics, Organizational Economics, FinTech and Textual Analysis, Consulting, Management, Non-Compete Agreements, Big Five, Fraud, Accounting, Disclosure.

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I Introduction

The design of principal-agent contracts shapes economic activity as studied in disparate fields such as labor economics, industrial organization, political economy, and corporate finance. In particular, the causes and consequences of contract incompleteness—the fact that parties do not to contract on all verifiable contingencies—have been an important focus of contract theory (e.g., see Hart and Moore (1988); Maskin and Tirole (1999); Hart and Moore (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, the flexibility of contract incompleteness can insure against enforcement risk (Gennaioli (2013)). Incompleteness might also be inevitable due to parties' limited cognition (Tirole (2009)). Despite a large theoretical literature, our empirical understanding of the drivers of contract completeness is still limited (Chiappori and Salanié (2003); Eigen (2012); Ganglmair and Wardlaw (2017); Gennaioli and Ponzetto (2017); Buchak (2016); Bond and Sigurdsson (2018); Jeffers and Lee (2019); Akey and Appel (2021)).

The extent of trust among contracting parties—the expected subjective probability that the counterpart will not engage in predatory conduct if unplanned states occurred after the contract is signed (Eggleston, Posner, and Zeckhauser (2000); Guiso and Makarin (2020))—seems a natural potential driver of contract completeness (Bottazzi et al. (2016)). If both principal and agent believe that the counterpart is unlikely to engage in predatory behavior in case unplanned states of the world realize, both parties might prefer to not engage in endless negotiations to plan for contingencies such as the stealing of confidential information or non-compete agreements. By contrast, if the principal's trust in the agent dropped she might insist to negotiate clauses whose contingencies courts can verify, thus enhancing contract completeness. Less trustful agents, too, might ask to negotiate additional clauses that clarify which behaviors should be considered wrongdoing during the contractual relationship.

Economically, trust is the subjective distribution of the probabilities the principal and agent attach to future predatory actions by the counterpart. A drop in trust obtains if either party's average expected probability of the counterpart's misconduct increases, either because of a higher perceived likelihood of each potential misconduct event or because the probability assessment becomes more uncertain—the parties becomes more "suspicious" that the other might engage in fraudulent or predatory behavior.

This paper aims to test empirically if and how trust among parties affects contracting. The first challenge this test faces relates to measurement: the econometrician needs to observe a large, representative, and homogeneous sample of principal-agent contracts in which the contingencies covered can be defined and measured meaningfully. 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 exploiting a quasi-exogenous shock to trust among parties in the context of consulting contracts between a principal (firm's shareholders/management) and an agent (consultant).

As we discuss in more detail in section II, we abstract from the principal-agent relationship between

shareholders and management in our setting. We characterize an effect of trust on contract completeness irrespective of whether higher contract completeness arises because both shareholders and management's trust in consultants drops or whether the management's trust in consultants does not change but managers know that shareholders are more distrustful of business practices with outsiders and hence require more complete contracts to avoid hostile actions by shareholders in case of agents' fraudulent behavior. In either case, as long as shareholders' trust in agents drops, managers might want to negotiate more complete contracts with consultants.

Our sample of contracts consists of all consulting agreements US public firms report to the Securities and Exchange Commission (SEC) through mandatory filings of material contracts. For each contract, we extract the principal and agent identities, whether the agent is a firm or an individual, 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.

The first contribution of our paper is to construct a set of text-based empirical measures the contingencies covered in contracts, which can be used broadly in other empirical applications of contract theory. In Section III, we discuss in detail how we measure the number of topics, unique words, and length of contract clauses. Number of topics, words, and length are meaningful proxies for contract completeness because contract clauses, contrary to narrative texts like news, display standardized structures and semantics. Adding a topic to a contract almost always means that the parties plan on additional contingencies. And, each unique word has a specific legal meaning that identifies one concept contrary to the frequent use of synonyms and rhetorical stylistic tools such as metaphors in narrative texts and news articles.

This first analysis provides a homogeneous and general assessment of contracts across space and over time, but does not allow us to disentangle the notions of completeness, complexity, and vagueness of contracts (Gennaioli and Ponzetto (2017)). In the second part of the analysis we thus focus on the content of individual clauses. We find that, after a negative shock to trust, the clauses that are added more frequently to contracts include non-compete agreements (only in US states that enforce them), confidentiality and indemnification clauses, termination clauses, and restrictions to equity bonuses to agents, all of which relate to general procedural rules that complete contracts rather than more complicated technical descriptions of the services agents perform.

To tackle the second empirical challenge—detecting a shock to trust between parties—we build on Giannetti and Wang (2016) and exploit the Arthur Andersen LLP (AA) scandal in 2002, which arguably represented the largest scandal in US accounting auditing and consulting. AA was one of the five largest auditors worldwide (*Big Five*) up until 2002. In 2002, AA was found guilty of obstruction of justice for destroying hard evidence about its audits of Enron. AA surrendered its CPA license in August 2002.

We show that small shareholders' (principals') trust in big business practices, which include consulting activities (agents), dropped substantially after the AA scandal (time variation) and by

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.

substantially more in US states in which more local listed firms were AA clients (spatial variation). As Giannetti and Wang (2016) document, more fraudulent behaviors were detected in states that were more exposed to the scandal after its realization in 2002, which aligns with the interpretation that local shareholders became more suspicious and less trustworthy of counterparts and started to detect and question predatory behavior more. Note that this argument does not require that shareholders are aware of who is consulting for their firms, but argues that, in general, after exposure to the shock all shareholders, including those who were not AA clients, are less trustworthy of their contracting counterparts, which pushes the management to change their contracting behavior with external parties.[1]

The left panel of Figure 1 portrays in the raw data the time series of trust in big business practices among college-educated US households—who are shareholders of public firms through their stock holdings—based on the Gallup Trust Survey. Shareholders' trust in both management and external consultants is likely to have dropped, because the AA scandal involved collusion of AA employees with Enron's management.[2] At the same time, the right panel shows that the completeness of consulting contracts has increased throughout the United States after 2002. This fact is a strong feature of our data and holds across several measures of contract completeness (see Figure 2). Moreover, we show that changes in the endogenous matching of parties cannot explain this fact because the average characteristics of firms and consultants did not change after 2002.

Although interesting, the evidence in Figure 1 cannot be interpreted causally. Any contemporaneous

[1] Consistently, as we discuss later, our results are virtually identical irrespective of whether we include firms that were AA clients or not. The source of variation we exploit represents a general drop in trust in external contracting parties for shareholders in states more exposed to the AA scandal, and not just a drop in trust in AA or any other specific party.

[2] Giannetti and Wang (2016) study the effects of this shock on shareholders' trust towards managers as captured by shareholders' decision to sell stakes in listed firms. Our analysis of contracts signed by firms over time by construction focuses on those shareholders who did not liquidate their holdings and hence trusted managers enough to let them keep negotiating contracts on their behalf.

time-varying shocks might explain the aggregate dynamics of both trust and contract completeness. 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 small shareholders' exposure to the AA scandal. We document that a higher state-level share of public companies that were AA's clients in 2000—where Giannetti and Wang (2016) show more fraudulent cases involving local companies were detected after the scandal—is associated with a larger drop in trust in big businesses practices in those states. Based on our interpretation, being more aware of the possibility of fraudulent behavior might have made local shareholders and managers less trustful and more suspicious towards agents and hence more careful in detecting potential fraudulent behavior. Importantly, this argument implies that the drop in trust should not be limited to shareholders and managers of direct AA clients, but to all shareholders in high-AA-exposure states, irrespective of the identity of their existing and future contracting parties.

We use this differential pre-scandal exposure to the shock across states to propose a difference-in-differences strategy. Our strategy compares the completeness of contracts signed on behalf of shareholders in states in which more or fewer shareholders were exposed to the shock before and after the shock. The design of this strategy relates to earlier work that exploits the spatial variation of trust within countries to disentangle the effects of trust from those of national formal and informal institutions.[3]

Our difference-in-differences analysis confirms that lower trust among contracting 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 clients relative to other states. Results are similar for several measures of completeness and across robustness tests. For robustness and completeness, we also show that the results are similar if we estimate instrumental-variable specifications in which we aggregate the measures of trust at the state-year level. These are not our main specifications, though, because the instrument defined at the state-year level is likely to be weak.

To validate our interpretation of the difference-in-differences results, we perform a set of falsification tests. We show that the share of state-level companies that were clients of *other* Big Five consulting firms does not explain trust in business nor predicts higher contract completeness. This test rules out that systematic shocks to the consulting services industry, such as new consulting contract templates implemented by big consulting companies, drive our results. We also show that the completeness of consulting contracts signed with firm insiders, such as C-suite managers, who are also principals through their shareholdings, does not change over time. Moreover, we show that the variation in exposure to the AA scandal has not affected beliefs about other institutions that could have induced higher contract completeness, such as trust in the judicial system or in banks. We observe these forms of trust directly as elicited at the same time and on the same college-educated households as the trust in big business practices and find no systematic changes after 2002 and/or across states.

[3]See, among others, Guiso, Sapienza, and Zingales (2004); Guiso, Sapienza, and Zingales (2008); Hilary and Huang (2016); Buggle (2016); Gurun, Stoffman, and Yonker (2017); D'Acunto (2019b); Pierce and Snyder (2017); D'Acunto, Prokopczuk, and Weber (2018); Levine, Lin, and Xie (2017); Depetris-Chauvin, Durante, and Campante (2020); and D'Acunto et al. (2020).

Firms that were AA clients before the scandal were forced to write new contracts after AA's demise and might have faced other shocks that made them sign more complete contracts. Contrary to this possibility, when we exclude all such firms our results do not change. Even non-AA client firms, and especially if located in the states in which the AA scandal that hit their peers was more salient (e.g., see Gurun et al. (2017); D'Acunto et al. (2019)), wrote more complete contracts after the AA scandal.

Another potential concern is that, after the AA scandal, firms started to disclose some material contracts they were keeping undisclosed before the scandal, which were systematically more complete.[4] Nation-wide new disclosure requirements such as those in SOX are dismissed by our difference-in-differences strategy. Against the possibility of state-specific unobserved shocks, we find that the average number of disclosed contracts, the average number of disclosing firms, and the average number of contracts disclosed within firms did not increase after 2002 and across states with a higher or lower exposure to the AA scandal.

In terms of alternative explanations, we find that the length of contract relationships did not change systematically over time or across states, which is not consistent with higher completeness substituting for a more limited role of relationship contracts after the AA scandal.[5] Moreover, firms did not stop to sign very short contracts, which might capture economic opportunities that by their nature do not allow the time to negotiate long contracts.[6] Also, the extent of civil lawsuits involving contract violations did not change over time and across states and hence contracts are not reacting to changing legal standards induced by potentially higher litigation in high-AA states' courts after the AA scandal.

After providing a broad analysis of trust and contracting with homogeneous measures, we move on to assessing which specific contract clauses are more likely to be added after a negative shock to trust in business counterparts.[7] This analysis is also important to understand if lower trust does increase contract completeness—that is, increases the contingencies that are explicitly discussed in contracts—or merely makes contracts more complex, for instance because consulting services and tasks in low-trust areas become more complex and are harder to contract upon (e.g., see Bond and Gomes (2009)).

We find that after the AA scandal and in locations with higher exposure, contracts were more likely to include non-compete clauses, clauses on the confidentiality of trade secrets and information, on indemnification, and on the procedures for the unilateral termination of the contract by the principal as well as for requesting amendments to the contract. Importantly, non-compete clauses appear more often only in contracts signed in US states in which such clauses are enforceable (Garmaise (2011); Jeffers (2018); Starr (2019)), which dismisses the concern that new boilerplate templates for contracts proposed by law firms or consulting firms can explain our results.[8] Overall, the newly appearing clauses refer to

[4] Note that this behavior would have represented a breach of the SEC regulation on the disclosure of material contracts.
[5] We thank Giacomo Ponzetto for proposing this alternative explanation.
[6] We thanks Luigi Guiso for proposing this alternative explanation.
[7] We thank Tracy Wang for inspiring this analysis.
[8] The addition of other clauses, instead, is similar across states that enforce or do not enforce non-compete agreements, which dismisses the possibility of unobserved shocks correlated with enforcement. We thank Rocco Macchiavello for suggesting this falsification test.

verifiable future contingencies that appear to reduce the extent of moral hazard on the part of agents (Akey and Appel (2021)) and support our interpretation of higher completeness rather than complexity.

Finally, we ask whether the effects we document are temporary or long-lived. In line with the notion that negative shocks to trust are harder to overcome than positive shocks (Schweitzer et al. (2006)), we detect no reversal of the effects. If anything, the effects build up over the first 5 years after the shock, perhaps due to the stickiness of contracts in place at the time of the shock.

Our setting faces the limitation that we cannot assess the role of trust on repeated contracting within the same relationships (Malhotra and Murnighan (2002); Lumineau (2017)), because we only have a small amount of contracts signed by the same firm and the same consultant before and after 2002 once we exclude mere deadline updates of the same contract. Moreover, whether the effects of distrust are triggered by shareholders' explicit pressure on managers to complete the contracts signed with (untrustworthy) agents or whether managers complete contracts to insure themselves against possible actions of more distrustful shareholders in case of a damage caused by agents are two channels consistent with our trust interpretation that we cannot disentangle and which beget follow-up research.

II Conceptual Framework and Data

The conceptual framework that drives our empirical analysis relies on the notion of *functionally complete* contract, that is, a contract to which parties cannot add any contingency because either the occurrence of such contingency would not be verifiable ex post, or it would be too costly to describe the state of the world under which such contingency arises (Eggleston, Posner, and Zeckhauser, 2000). A functionally incomplete contract is thus a contract to which ex-post verifiable contingencies can be added.

Incomplete contracts can arise either because of transactions costs (Williamson (1985); Hart and Moore (1988)), such as the costs of negotiating contract clauses, or because one or both parties lack the cognitive abilities needed to foresee and describe all potential future verifiable contingencies (Maskin and Tirole (1999); Tirole (2009)), or because the parties enjoy mutual benefits from avoiding to bargain on all potential contingencies at the start of the contractual relationship (Crocker and Reynolds (1993); Halonen-Akatwijuka and Hart (2013)).

Limited foreseeing of potential future contingencies is an unlikely driver of incompleteness in our setting. Indeed, we study the material consulting agreements of listed public companies, which represent a traditional area of business activity for these companies. As we discuss and analyze empirically below, the contingencies more complete contracts cover in our setting relate to standard restrictions to agents' actions, such as the punishment of stealing of proprietary information and non-compete agreements. That the management of public companies might lack the cognitive abilities to foresee the possibility of a future state of the world in which a consultant steals the firm's proprietary information is rather implausible. For instance, the possibility of violating the secrecy and confidentiality of data and information acquired

within a principal–agent relationship has been part of private contracting for centuries (e.g., see the notion of *actio servi corrupti* in Roman Law).

A relevant option value from postponing the negotiation of some contingencies is also unlikely in our setting because of both the substantial difference in bargaining power between the principal—a large public firm—and agent—an individual consultant as well as the rather standard type of consulting services agents provide in our context.

Ultimately, transaction costs are the most plausible driver of contract completeness in our setting. If parties face transaction costs when negotiating contracts, the trade off between the costs and benefits of negotiating on each potential future state of the world, even when perfectly foreseeable, should determine whether such state is discussed in the contract. Principals' expected probability that a negative state of the world might arise is a crucial determinant of their decision about whether to negotiate contracts that plan for each potential future state of the world.

For instance, suppose that the principal is considering whether to negotiate detailed provisions with the agent regarding the assessment and procedures in case the agent uses the firm's proprietary information outside of the contractual relationship. Even if the principal fully understands that this potential state of the world is a non-zero-probability event, if the principal believes that the probability of this state is very low, he/she might prefer to not engage in endless negotiations with the agent on such sensitive issue given that the expected benefits of adding the clause are low due to the low expected probability that the negative state arises. The opposite would be true in an environment in which the principal's subjective expected probability that the agent might steal proprietary information is higher. In this case, if this expected probability is high enough, the principal will want to pay the costs of negotiation ex ante given that the expected benefit from avoiding a non-negligible positive probability negative state is now higher.

Similar considerations hold in terms of a prospective agent's trust towards the principal. Agents, too, might request the negotiation of additional contract clauses that specify which facts and actions might represent wrongdoing on their part if they were worried that principals could accuse them of wrongdoing for acts agents deem acceptable.

The transaction-cost setting brings us to the definition of trust between a principal and an agent. Trust in our setting is the set of expected probabilities the principal attaches to potential negative future states of the world caused by the agent, and vice versa. A principal trusts the agent more if he/she believes that the probabilities the agents steals proprietary information, engages in undue competition and stealing of customers, or other negative actions against the principal are all negligible. A principal's trust in the agent declines if the principal's expected probabilities of such negative states of the world caused by the agent increases.

Our research design will take the initial level of trust between principals and agents as given and will exploit a negative shock to trust that affected parties differently over time and across space. We will

interpret this shock as quasi-exogenous variation in the extent to which parties updated their expected probability of a negative state caused by the counterpart upwards—a negative shock to trust between principals and agents. After the shock, the most affected principals thus believe that the likelihood of a negative state of the world due to the agent's action is higher than before the shock, and vice versa. For this reason, the expected benefit from negotiating more complete contracts would increase and might become higher than the costs of negotiation, thus inducing principals and agents to negotiate more clauses and make the contracts more complete relative to before the shock.

Another important part of our conceptual framework is the mapping of the roles of principals and agents to the actors in our setting. Agents are prospective external consultants who are hired for material services by US listed firms. Principals are both the shareholders and managers—who negotiate contracts with external consultants on the shareholders' behalf—of public listed firms. Here, we abstract from the principal-agent relationship between shareholders and managers and assume that, after the negative shock to trust, managers negotiate contracts with external consultants based on the residual level of trust shareholders have in external consultants. This means that the management's trust in the prospective agents might decrease after the negative shock. But even if it did not, managers would still have an incentive to complete contracts to avoid shareholders' action against them in case the agent misbehaved and such negative contingencies were not planned in the contract. For this reason, we will focus on shareholders' trust in external business services, which we can measure in our data.

Finally, note that our setting does not require that the small and often inattentive shareholders of public companies follow the negotiation of contracts with external consultants directly, which is obviously a standard operational activity in which the management engages on behalf of shareholders. Shareholders' trust is important because if a negative contingency realized—whose realization will be known by shareholders ex post—and the management had not planned for such a contingency, the management might face shareholders' indemnification action. Managers would not be able to argue that the possibility of agents' misconduct was hard to foresee. The management thus has an ex-ante incentive to align the conduct of contract negotiations with agents to the level of trust shareholders have in such agents.

III Institutional Setting and Measuring Contract Completeness

In this section, we describe the construction of our novel sample of consulting contracts and how we propose mapping contract features into measures of contract completeness. Because our baseline measures might capture at the same time contracts' completeness and complexity, in Section VII we propose a complementary analysis that studies quantitatively and qualitatively the content of the clauses that are added to contracts after a negative shock to trust.

A. 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.[9] The firms whose contracts we observe are regulated by the SEC, and hence are either public firms or private firms that issue public debt instruments in the US.

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 to announce major events about which shareholders should be informed. Firms are required to file such form within 4 business days from the occurrence of the event. In the case of consulting contracts, the full texts of such contracts are attached to Forms 8-K and made public. In the case of annual and quarterly financial reports, the full contracts are also attached to the form. The main difference between the reporting vehicles is the timing of the disclosure of information. If the firm chooses to disclose and make public the contract in an 8-K filing, the information is disseminated when the contract is signed. Under 10-Q or 10-K filings, the information is public only at the end of the quarter or end of the fiscal year. Of course, firms might have several 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 out results separately for both types of reporting vehicles.[10]

To identify and have access to all 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.[11] 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 334988, 266198, and 304674 Exhibit 10 sections as attachments in 8-K, 10-K, and 10-Q filings.

To narrow the scope of our search to a sample consulting agreements, we identify 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)

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

[10] We do not claim that the choice of reporting vehicle is in any way exogenous, but we show that as far as the completeness of contracts is concerned, in our difference-in-differences analysis the results do not change based on the endogenous choice of reporting vehicle.

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

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 in our identification strategy; and (iv) contracts for which we cannot obtain the *gvkey* through the "List of Filings Exhibits", for which we would not know 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 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 consultants' names, 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 residence.

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, the choice of state for governing laws, and the total and unique number of words and sentences.

B. Mapping Contract Features into Measures of Completeness

A major challenge to tackle our question is defining the mapping of the concept of contract completeness into a variable we can measure in the data. Ideally, we would be able to 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 principals' clients. Whether an agent engages in competition against the principal is verifiable, and hence adding a non-compete agreement to the contract increases its completeness as well as its complexity.

Based on these considerations, we propose two empirical approaches to measure contract completeness. The baseline approach is based on complexity, under the assumption that a more complex contract might weakly be more complete to the extent that at least one of the additional contingencies in the contract can be 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 approach based on whether, above and beyond the complexity of clauses, a contract includes verifiable contingencies instead of unverifiable contingencies and hence is more complete despite not being more complex. For this approach, we move the analysis to the clause level and assess which clauses tend to appear more often over time.

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).

C. Topic-Modeling Measures of Contract Completeness

Our first set of measures of contract completeness are based on the amounts of topics contracts cover. By construction, covering more topics means 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 consulting contracts, relative to prosaic texts and other narrative forms, because of the homogeneity and standardization of their structure.

The main challenge to count the number of topics covered in contracts is defining a consistent, coherent, and systematic definition of potential topics and methodology to assign groups of words to alternative topics. Manual coding is barely appropriate, because the detection of alternative topics might be subjective to the coder, it would be hard to guarantee homogeneity, and other issues Huang et al. (2018) discuss.

To tackle this challenge, we resort to textual-analysis techniques and 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.

C.1 Implementing the Topic-Modeling Approach and Examples

To analyze the topic structure of consulting contracts, we use the universe of 8,788 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 in terms of their meaning, as well as a probability attached to each word, which captures the probability that the word is indeed semantically related to the other words within the topic.

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 crucial elements of each topic—the words that are related enough to constitute a topic and the probabilities attached to each word (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 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. Looking actively for words barely related to controversies and checking their 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.[12] A second caveat to keep in mind is that some of the words that enter the topic 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 VII, we discuss in more depth additional topics and especially the topics that we find increasing in contracts in our difference-in-differences analysis.

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

[12]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.

sample of consulting contracts.

An important feature of the LDA procedure is that it requires an upfront decision about the optimal number of topics the researcher wants the procedure to identify in the universe of contracts available. To inform this assumption, we compute the *perplexity score* proposed by Huang et al. (2018).[13] As a 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 ensure that our results are unrelated to this assumption, for robustness we also use a low value of 100 optimal topics and a high value of 300 optimal topics as alternative assumptions and construct the measures separately based on these assumptions.

After having identified the set of 200 optimal topics based on the universe of contracts, the second part of the procedure computes the number of topics (among the 200) each contract includes. 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 topics the LDA identified in the first part based on the similarity of the words in the sentence to the words that enter each topic. We then sum up the number of unique topics within each contract.

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

D. 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 propose is not transparent enough, or that other contract features might also proxy for completeness.

To tackle these concerns, we also propose a set of measures 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 it is that the contract disposes for more potential future states of the world. This is plausible, because the structure of consulting contract clauses is homogeneous, and the discussion of each contingency uses homogeneous semantics. Differences in contract length are thus unlikely to capture different writing styles, as would be 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 use textual-analysis algorithms that simply count the words and sentences in contracts.

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

E. Summary Statistics

Table 1 reports summary statistics for our sample. Panel A shows the number of contracts for each firm in the sample and across different types of contracts. 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. 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 of the contracts (N=4,150) are in the treatment period, after the AA scandal, between 2003 and 2015 included (13 years). Moreover, in about two thirds of the sample agents are outsiders (N=4,067). The baseline analysis will focus on contracts with outsider agents, and we will propose a placebo test that consider insiders' contracts.[14]

Because of the novelty of our contract sample, we present the distribution of contracts across US industries (Table A.1) and across US states (Table A.2). We will keep variation across these two dimensions constant in our baseline analysis using fixed effects, which makes these (interesting) sources of variation irrelevant to our results. In Panel B of Table 1, we describe the variables at the contract level, which is our unit of observation. State-level shares of AA clients are defined more broadly but enter our analysis at the contract level. The share of clients of the *Big Five* consulting companies appear homogeneously distributed across space, and each *Big Five* covers between 11% and 15% of all contracts.[15]

We then summarize our main outcome variables—the measures of contract completeness defined above. We excluded the shortest contracts when computing these measures, because we do not have enough sentences to meaningfully apply our textual-analysis procedure. *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 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 through 8-K forms. Moreover, 29% of the agents are legal persons (*Company*). Finally, 32% of all contracts include stock option payments and about 42% include stock grants. Overall, about two third of all contracts include at least one form of equity incentives.

[14] As we discuss in more detail below, by construction shareholders trust company insiders, typically board members, management, or other employees, in the same way as they trust the management to negotiate consulting contracts on their behalf.

[15] These shares do not sum up to 1, because of individual contractors and smaller consulting firms.

IV Shock to Trust in Big Business Practices

In this section, we describe and validate the drop in trust between principals and agents we use in our difference-in-differences analysis.

A. Enron Scandal and the Demise of Arthur Andersen

Enron Corporation was a Texan energy company. This public company was involved in one of the largest accounting fraud scandals in history—the Enron scandal—which led to the bankruptcy of the company. According to trial evidence, the company had hidden billions of dollars in debt from failed deals and projects for years, making wide use of a set of accounting loopholes, special purpose entities, and poor financial reporting.

Arthur Andersen LLP (AA)—Enron's auditor and consultant—did not report these misguided practices. On March 5, 2001, Bethany McLean, an American journalist, was the first public voice to raise doubts about Enron's financial accounts, 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.

Apart from Enron's management, AA, auditor and consultant, was accused of negligence and fraudulent behavior. On January 17, 2002, Enron dismissed AA accusing them of fraudulent consulting 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 if 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 non-consulting operations. Part of AA's consulting activities continued through Accenture.

B. Drop in Trust in Big Businesses Practices

The AA scandal was a form of business malpractice based on the contractual relationships of big corporations with external consultants. A generalized drop in small shareholders' trust in the practices of large corporations seems plausible. And, indeed, as we showed in the left panel of Figure 1, the public's trust in big businesses practices dropped substantially after 2002 and stayed low throughout our sample period.

Because the public includes the vast majority of public companies' shareholders, this drop in trust has had two effects. The first effect operates on the extensive margin, whereby shareholders decide to exit their investment in public companies due to their drop of trust in the management. Giannetti and Wang (2016) document and analyze this effect. At the same time, a contemporaneous effect might operate through the intensive margin—those shareholders who decide not to exit must trust the management

enough to not liquidate their shares, and yet their trust in business practices, including the relationships with external consultants, among others, might have dropped. Based on our discussion in section II, the AA scandal might have reduced shareholders' trust in business practices because the salience of a major instance of misconduct by agents increased shareholders' subjective expected probability of agent misconduct relative to before the scandal. The same scandal might have reduced agents' trust in principal and hence increased their request to discuss explicitly which states of the world would constitute wrongdoing during the contractual relationship.

Our setting by construction speaks to the intensive-margin effect of the drop in trust in big businesses' practices on the part of small shareholders who do not liquidate their holdings and hence still trust the company's management. Our measure of trust in business practices by small shareholders does not necessarily need to be highly correlated with managers' trust in such practices (which is unobserved). As we discussed in section II and in the introduction, in our framework we define an effect of trust on contract completeness irrespective of whether higher contract completeness arises because both small shareholders and management distrust consultants after the AA scandal or whether the management knows that small shareholders are more distrustful and hence require more complete contracts to agents to avoid actions by shareholders in case of subsequent misconduct.

The raw-data, time-series evidence in the left Panel of Figure 1 is not sufficient to conclude that the AA scandal was a shock to trust in business practices. On the one hand, other contemporaneous shocks might have contributed to this drop in trust. On the other hand, the scandal might have affected other aspects of shareholders, management, and consultants' preferences and beliefs.

B.1 Variation in Exposure to the Scandal Across US States and Trust

To assess these concerns, we propose several tests. The first test builds on Giannetti and Wang (2016). We exploit cross-sectional variation in the salience of the shock to shareholders. Specifically, we use the state-level share of public companies that were AA clients in 2000—before any concern about potential wrongdoing related to the AA scandal erupted—as a proxy for the salience of the scandal among local shareholders, which, because of the home bias phenomenon, are overrepresented among the shareholders of local public companies (e.g., see Coval and Moskowitz (1999)). Giannetti and Wang (2016) find that fraud cases unrelated to AA and Enron were more likely to be detected and prosecuted in higher-exposure states, which would be naturally explained by the fact that trust towards business practices dropped more in those states—principals lowered their subjective probability that potential agents would not take predatory behaviors against them—and hence principals started to monitor business and contract relationships more closely, thus increasing the detection of potential fraudulent behavior on the part of agents. The salience channel our interpretation proposes, that is, the possibility that the beliefs and trust of shareholders in locations that were more exposed to the scandal reacted more than others' trust, is also consistent with earlier research that has documented this type of channel in the US and abroad (e.g.,

see Gurun et al. (2017), D'Acunto (2019a), and D'Acunto et al. (2019)).

Note also that our proposed salience channel does not require that small shareholders were aware that AA were the consultants and auditors of their firms, which is quite implausible. Rather, the argument is that in states where local firms were more exposed to AA, where more fraudulent behaviors were detected after the scandal (Giannetti and Wang (2016)), local small shareholders, irrespective of whether they owned shares of AA-client firms or not, became more suspicious of business practices. Not only the small shareholders of firms that were AA clients thus should have reduced their trust in business practices, but the average of all small shareholders in states in which the scandal was more salient. This is the reason why we will consider the contracts of all firms in high-AA-exposure states rather than just the contracts of firms that were direct AA clients when moving to assessing the effects of trust on contracting.[16]

Figure 4 is a state-level heatmap of the variation of the share of AA clients in 2000. The darker is a state, the higher is the share. We detect no obvious spatial clustering: Substantial variation exists within the group of large states that include many US public companies, such as California, Texas, New York, Massachusetts, and Illinois as well as within the group of smaller states across all US regions. We also report the state-level shares of clients of other Big 5 consulting firms in Table A.3 of the Online Appendix.

Based on the raw spatial pre-exposure to AA of shareholders across US states before the scandal, we test whether the state-level variation in the share of AA clients helps explain the variation in the public's trust of large corporations' business practices using the individual responses for waves of the Gallup Trust Survey between 1990 and 2015 (see Giannetti and Wang (2016) for a detailed description of this source). We estimate the following linear specification:

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

where $Trust_{i,s,t}$ is the amount of trust in the business practices of large corporations by respondent i in state s in year t; $Treated_s$ 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 clients of Arthur Andersen in 2000;[17] $After\ 2002_t$ is an indicator variable that equals 1 for the period 2003-2015, and zero for the period 1994-2002; X is a vector of respondent-level characteristics that include 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. We limit the sample to respondents who

[16] If anything, we will show robustness tests that exclude the contracts signed by direct AA clients after the demise of AA to verify that our results are not driven by firms that were forced to rewrite their consulting contracts, given that they needed to change consultants.

[17] We only use the nonlinear version of the treatment in this table due to space constraints. Results are similar when using the continuous version (share of AA clients in state s in 2000). Below, for the results on the effects of trust on contract completeness, we report the results when using both treatment measures.

have at least a college degree, because shareholding is much more likely for college-educated individuals and hence these respondents are more likely to capture the beliefs of public companies' shareholders. We estimate equation (1) by weighted least squares, in which we weigh observations by the number of respondents in each state.[18]

Columns (1)-(3) of Table 2 report the results from estimating equation (1). In column (1), we find that trust in big businesses after 2002 decreased in all US states, but more so in states whose share of AA clients among local public companies was above the 75th percentile before the scandal. For other states, trust decreased by 4.5 percentage points, whereas for states with the highest share it decreased by about 100% more. Overall, trust in big business practices dropped by about one quarter of the average level of trust in the full sample (31.4%). In columns (2)-(3), we add year and state fixed effects to ensure that our baseline result is not driven by business cycle shocks common to the whole US or to time-invariant characteristics of US states. The results are quantitatively and qualitatively similar. Overall, the share of AA clients across US states in 2000 is negatively associated with the extent of trust in big businesses by local households, who are more likely to be found among the small shareholders of local public firms than others.

B.2 Falsification Tests: Share of Clients of Other Consultants and Different Forms of Trust

The results so far cannot rule out two alternative explanations. First, general trends in the consulting industry might drive the patterns in columns (1)-(3) of Table 2; for instance, local firms might start choosing small consulting shops over large consulting firms (Big 5) after 2002. 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, we should find similar estimates as those discussed above. Instead, in columns (4)-(7) of Table 2 we fail to reject the null hypothesis that the trust in big businesses by local small shareholders did not change across states based on the 2000 share of clients of other Big 5 consultants. The coefficients are, if anything, positive, although economically small and statistically insignificant.

A second concern is the Enron scandal might have also affected shareholders' 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. We

[18] Given the low number of respondents in some smaller states, the representativeness of their answers might be lower and hence we put less weight on those observations.

can test directly whether the variation in the AA share predicts variation in trust towards other relevant institutions. We do so in columns (8)-(10) of Table 2, in which 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 change in the trust in these institutions 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 relative to other large consulting firms, and does not percolate to other relevant institutions.

V The Effect of Trust on Contracting

Armed with the negative shock to trust between shareholders and business practices, including consulting practices, we move on to estimate the effect of a drop in trust among parties on contract completeness.

Our primary strategy is based on a difference-in-differences reduced-form specification, in which we compare the completeness of contracts before and after the AA scandal and across states that were more or less exposed to the scandal. We focus on this strategy because, as we discuss and show below, designing an instrumental-variable (IV) analysis requires us to make arbitrary assumptions about how to aggregate the survey-based individual-level trust observations at the state level and across most assumptions delivers us a weak instrument. The IV results are fully in line with the diff-in-diffs results both economically and statistically, but because of the weakness of the instrument when the endogenous variable is measured at the state level, we only run them for completeness and robustness.

A. Reduced Form Difference-in-Differences

We start by estimating our specifications in reduced form, which does not require us to make assumptions about how to aggregate the individual-level trust observations at the state level. Later, we propose two-stage least-square results that use an aggregate measure of trust at the state-year level as the endogenous variable:

$$Completeness_{i,j,s,p,t} = \alpha + \beta\ Treated_s \times After\ 2002_t + X'_{j,s,p,t}\delta + \eta_t + \eta_p + \eta_s + \epsilon_{i,j,s,p,t}, \qquad (2)$$

where $Completeness_{i,j,p,k,t}$ measures completeness of contract i signed by firm j in state s 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 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 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

[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.

otherwise.

We add a full set of year fixed effects (η_t) to absorb US-wide business cycle and regulation shocks, industry fixed effects (η_p) to ensure that systematic differences in contract completeness due to the nature of business activity do not drive our results, and state fixed effects (η_s) to absorb state-level legal and regulatory characteristics invariant over time, which might affect both contract completeness and the pool of available consultants in a state. The sample period is between 1994 and 2015 and the sample includes all the contracts we observe signed between a firm and outsider consultants, because trusting insiders, who are shareholders and/or management, should not change in our setting. We formally test this argument in the falsification analysis below.

To estimate equation (2), we cannot use a linear estimator like OLS, because the outcome variable $Completeness_{i,j,s,p,t}$ is a count variable, a set of natural numbers, and OLS estimates would be biased in unknown directions. We use a negative binomial estimator, which accounts for the count nature of the outcome variable and allows for potential overdispersion in the distribution of contract completeness. We cluster standard errors at the level of the state, which allows for correlation of unknown form across the observations of contracts in the same state over time. Because our data include 23 years, we do not cluster standard errors also at the year level due to the small number of clusters, which would likely result in less conservative estimates.

Because our main covariate of interest, $Treated_s \times After\ 2002_t$, varies at the state-year level, our specifications cannot include a full set of state-year fixed effects to absorb potentially different business cycle shocks or other contemporaneous shocks at the state level that might explain contract completeness and might be stronger in states with a higher pre-share of AA clients, on top of the AA scandal. In the most complete specifications we instead add a set of firm-level time-varying controls that might explain contract completeness within firms over time.

We add the set of contract-level characteristics we can observe in our data. $8KReporting$ is a dummy that equals 1 if the firm reported the consulting contract in an 8-K filing, as opposed to the 10-K or 10-Q filings. Contracts reported through 8-K forms might in principle differ systematically from other contracts. If firms move to 8-K reporting over time and especially after 2002, we need to absorb this variation. $Company$ is a dummy that equals 1 if the consultant is a legal person, and zero if it is an individual. $Arbitration$ is a dummy that equals 1 if the contract chooses arbitration to solve disputes relative to accessing to the judicial system. By construction, choosing arbitration requires more features to be discussed in the contract and is mechanically associated with higher completeness. We also add two proxies for firms' performance and operations risk—$Return$ and $Volatility$—which correspond to the average return of the firm's stock and the volatility of the firm's stock at each end of fiscal year. Finally, we control for the logarithm of states' yearly GDP as a direct proxy for time-varying business cycle shocks at the state level.

We report the baseline estimates of equation (2) in Table 3. The estimated coefficient attached to

$Treated_s \times After\ 2002_t$ is stable across specifications in columns (1)-(3) as we add fixed effects and controls to the analysis, which reduces concerns that unobserved heterogeneity might explain the results. Across all specifications, the sign of the coefficient is positive and statistically different from zero, which suggests that a drop in trust between shareholders and business counterparties leads to an increase in contract completeness.[20]

To interpret the magnitude of these coefficients, recall that we use a negative binomial estimator. The coefficients provide the log-difference in the levels of the outcome variable for observations in which the dummy variable equals 1 and other observations. For the case of $Treated_s \times After\ 2002_t$, for instance, after 2002, the logarithm of the number of topics in contracts signed by firms at the top of the state-level distribution by AA clients is 0.101 higher than the logarithm of the number of topics in other contracts both before 2002 and across firms in states with a lower pre-shock share of AA clients.

To assuage this effect, we compare it to the average value of the outcome variable in the sample, which is 22.96 topics. The natural logarithm of 22.96 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 clients 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, as captured by the estimated coefficient in the table).

Other controls in column (3) that by construction should capture more complete contracts—*Arbitration* and *Company*—indeed display a positive association, which is reassuring given that our sample and the proposed measures of completeness have not yet been validated by other research.

In columns (4)-(6) of Table 3, we estimate equation (2) using the continuous value of the 2000 state-level share of AA clients. The sign, statistical significance and magnitude of the estimated coefficients are similar. The coefficient attached to $Treated_s \times After\ 2002_t$ is the change in the logarithm of the number of topics in states with a share of AA clients of 1 relative to a share of AA clients of zero. The value of 0.635 in column (6) implies the number of topics covered is 43.16 - 22.96 = 20.2 higher in the former group. Moving from the 25th percentile to the 75th percentile of the AA share is an increase of 0.068, which corresponds to 1.37 more topics covered—about 6% of the average number of topics covered by the contracts in our sample.

B. Robustness: Measures of Completeness and Alternative Specifications

In Table 4, we verify 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 clients 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 clients in states as of 2000.

In Panel A, 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,

[20] In all specifications that include state fixed effects, the level of the treatment variable is absorbed.

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 B and C, 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 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.[22] 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 words and sentences in each contract, either in full or unique. The positive effect of a drop in trust on contract completeness is replicated when we use the count-based measures.

C. Falsification Tests

To further corroborate our interpretation of the baseline results, we perform a set of placebo and heterogeneity tests. We start by considering consulting contracts with firms insiders. These contracts are often signed to provide an alternative form of payment to insiders or to ensure delayed severance payments to retired managers and hence are usually mere vehicles for compensation above and beyond insiders' formal compensation. Trust should have barely any role in the design of these contracts, because most firm insiders are also principals (shareholders) through their stock holdings. Even in the rare cases of managers or board members that own no stocks of the company, shareholders trust such managers and board members enough to let them manage the company.

We estimate equation (2) on contracts between the firm and an insider. Insiders include board members, management, or other employees of the firm. Columns (1) and (6) of Table 5 show that, consistent with our conjecture, contract completeness does not differ systematically for insider contracts. This non-result holds true in terms of both economic and statistical significance. 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 economic or regulatory contemporaneous shocks, which should have affected the design of all consulting contracts, including insider contracts.

The second falsification test considers the state-level share of clients of other *Big 5* consulting companies. Because these shares, as we showed earlier, do not predict a drop in trust, based on our interpretation we should find no effect on contract completeness either. Otherwise, we would have direct evidence that channels different from trust in consultants drive at least in part our baseline results.

In Table 5, columns (2)-(5) and columns (7)-(10) corroborate this conjecture. If anything, for one

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

[22] This fact should not come as a surprise, because the summary statistics of the distributions of the three topic-modeling variables in Table 1 are quite similar.

of the other companies, Deloitte, we find a significant negative coefficient. This result, though, is not replicated in other specifications that change the set of fixed effects and controls.

D. IV: Two-Stage Least-Squares Specifications

So far, we have considered a reduced form difference-in-differences specification. As we briefly discussed at the beginning of section V, this specification has a set of advantages relative to an instrumental-variable approach. First, it does not require us to make arbitrary assumptions on how to aggregate the individual-level survey-based observations of trust in big businesses practice at the state level, which is the finest common level of observations between the trust data and the contract data. Second, the instrument for trust at the state level might be weak, which, as we show in more detail below, appears to be the case in our setting.

The main disadvantage of the reduced form diff-in-diffs is that it estimates the causal effect of exposure to the AA shock before and after the shock rather than the causal effect of a change in trust on contract completeness. Above, we described several direct and falsification tests to support the assumption that the AA scandal affected local trust in big businesses practices rather than other channels, but these analyses cannot definitely rule out additional unobserved channels, even if implausible.

For the sake of robustness and completeness, we thus also report the results from an IV analysis—a set of two-stage least-squares specifications. We first aggregate the individual-respondent level observations for trust at the state level. To do so, we compute the logarithm of the average share of respondents that report a high level of trust in big business practices. In a first version, we only consider college-educated respondents to be consistent with the analysis of trust in section IV, because college-educated respondents are more likely to be shareholders of listed companies through their stockholdings. For the sake of robustness, we also compute the measure when considering all respondents, regardless of their education level. Because the original survey is not stratified at the state-year level, the information in these averages will be more accurate for the states in which we have a larger proportion of observations. For this reason, we weigh state-level average trust by the number of college-educated respondents and by the number of total respondents in each state-year, respectively.

To define the instrument for state-level trust, we also consider two alternatives for completeness. On the one hand, one might argue that the main source of quasi-exogenous variation in trust come from the time variation between before and after 2002, across states that were differentially exposed to the shock. In this case, we would use only the interaction between the share of AA clients in the state in 2000 and the dummy variable for after 2002 as the instrument for the state-year level of trust (including the share of AA clients as a control in the first and second-stage regressions). On the other hand, both the ex-ante variation in exposure to the AA scandal as well as its interaction with the dummy variable for after 2002 could be used to instrument the state-year level of trust. For completeness, we consider both options.

We implement the IV analysis by estimating the following two-stage least-squares specifications,

where equation (3) represents the first stage and equation (4) the second stage:

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

$$Completeness_{i,j,s,p,t} = \alpha + \beta\ \widehat{Trust}_{i,j,s,p,t} + X'_{j,s,p,t}\delta + \eta_t + \eta_p + \epsilon_{i,j,s,p,t}, \quad (4)$$

The exclusion restriction we need to assume if we wanted to interpret the IV results causally is that exposure to the AA scandal only affected contract completeness after the scandal through its effect on the lower trust by firms' shareholders/management towards agents, rather than through other channels. This exclusion restriction is untestable, even though the evidence on the direct effect of the AA scandal on trust in big business practices and the falsification tests on other channels we described in section IV go in the direction of supporting the plausibility of this assumption.

Table 6 reports the results for estimating equations (3) and (4). On the one hand, the results in Table 6 show that both the first stage and the second stage go in the direction we conjectured. In the first stage, the state-year level of trust (which are repeated observations for each firm-year, which is the level of observation for the second stage) is predicted negatively by exposure to the scandal, after the scandal. In the second stage, when instrumenting state-year levels of trust with the variation in exposure to the AA scandal, we uncover a negative effect of trust on completeness, i.e. completeness increases in lower-trust environments, exactly as we saw in the reduced-form diff-in-diffs analysis. The results are quite similar across the various measures of state-year-level trust and weighting schemes.

On the other hand, Table 6 shows that our instrument for the state-year level of trust is weak. The first-stage F-statistics across all specifications are low. The weakness of this instrument is the main reason why we do not consider the IV analysis as our main empirical specification but we report the results for the sake of completeness and robustness.

VI Alternative Channels and Explanations

In this section, we propose a set of empirical tests to assess alternative channels and explanations to our baseline results.

A. Characteristics of Disclosing Firms, Agents, and Contracts over Time

The first set of alternative explanations we consider is that, after the AA scandal, the types of principals, agents, and/or contracts we observe in our sample have changed systematically even if contracting practices have not.

First, certain types of firms that were not disclosing their contracts before the scandal, perhaps because of their high complexity, might have started to disclose more contracts after the scandal. In this case, the increase in observed contract completeness would be due to the fact that more complete

contracts existed but were not disclosed before 2002. Note that this explanation can only fit our results if firms in high-AA states were prone to disclose more after 2002 than otherwise similar firms in low-AA states. Contrary to the possibility that the pool of disclosing firms changed, in Panel A of Figure 5 we find that characteristics such as the size of disclosing firms (number of employees), their leverage, or the share of high-tech-industry firms over all disclosing firms does not follow the same patterns we uncover for trust in big business practices and for contract completeness. Moreover, we consider the intensive margin of disclosure—whether the same set of firms started to disclose complex contracts they were not disclosing before 2002. We show in Figure A.3 of the Online Appendix that this alternative explanation is also unlikely, because the average number of contracts disclosed per firm in our sample did not change around 2002 and stayed flat throughout our sample period.

Second, one might wonder whether the pools of consultants have systematically changed across states and over time based on the AA scandal, and because of this change rather than to a drop in trust principals might have started to write different contracts. In Panel B of Figure 5, we find that this explanation is quite unlikely, because for the set of characteristics of consultants we can observe (whether consultants are individual contractors rather than employees of consulting firms, whether consultants are external or internal, and whether consultants are CEOs of their companies) we detect no systematic differences around 2002.

B. Dismissal of Relationships That Do Not Allow Completeness

We move on to consider another alternative channel that we label the *selection channel*.[23] In this case, selection does not relate to the pools of contracting parties we observe in our sample but to the types of contractual relationships that are pursued and codified in contracts before and after the AA scandal. Specifically, after the AA scandal, firms might have simply dismissed a set of potential business opportunities and contractual relationships that, due to their nature, would 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 start to be avoided by the firms most hit by the AA scandal. Consequently, the set of contracts in our sample would be selected towards more complete contracts after 2002 and in high-AA states.

The evidence in Figure A.3 of the Online Appendix provides indirect evidence against this potential explanation, because under this channel the average number of contracts disclosed by firms should be lower after the scandal, and we find it is flat throughout our sample period. For a more direct test, we consider the share of disclosed contracts that are very short. Intuitively, the types of contractual relationships for which substantial negotiation of contracts is not practical should be codified in very short contracts. In Panel A of Table 7, we repeat our baseline analysis when using a dummy variable for

[23] We thank Luigi Guiso for proposing this channel and suggesting an empirical test.

whether a disclosed contract is among the shortest in our sample.[24] We find no evidence that very short contracts are more or less likely to appear in our data set after 2002 and/or in high-AA states.

C. Relational Contracts vs. Trust

We also assess the possibility that principals revised their beliefs about the length of contractual relationships after the AA scandal. If contractual relationships were expected to be shorter after the Enron scandal, relational contracts would have not been a substitute for (formal) contract completeness anymore (Baker et al. (2002), Macchiavello (2018), Macchiavello and Morjaria (2021)). Principals might have thus started to push agents to write more complete consulting contracts irrespective of their trust in agents.[25]

To assess this explanation, we test whether the variation we use to explain contract completeness can also explain the length of contractual relationships. If this was not the case, the scope for a muted role of relational contracts after the Enron scandal would be low—it would only exist if principals were making systematically wrong inference about the expected length of contractual relationships.

Panel B of Table 7 reports the results for regressing the length show that the variation in the salience of exposure to AA around 2002 does not explain the average duration of contracts as stated at the time of signing, which suggests that the possibility that principals thought they had to plan for shorter relationships when signing more complete contracts is an unlikely explanation for our results.

D. Changing Legal Standards Due to State-specific Litigation

Finally, we consider the possibility that the AA scandal induced more litigation and civil lawsuits in high-AA states and the court decisions created new legal standards to which local public companies, but not others, had to abide. Managers in high-AA states might have incorporated new clauses in consulting contracts due to such state-specific changing legal standards.

To assess this channel, we collected data from the *Federal Judicial Center's* (FJC) Integrated Database, which includes information on all federal, civil, criminal, bankruptcy, and appellate court cases reported to the *Administrative Office of the US Courts*. We use information on the type of lawsuits, their object, and the parties to restrict the sample to all civil lawsuits that involve an issue of contract violation. For each state-year, we compute the overall number of civil lawsuits involving contract violation, the number of such lawsuits that involve a local public firm, and the share of all civil lawsuits involving contract violation that also involve a local public firm.

The channel we want to assess could explain our diff-in-diffs results if the number of civil lawsuits involving contract violation changed differentially after 2002 across states with different AA exposure. We

[24] We define a contract very short is the number of unique words in the contract is below 100, which typically corresponds to less than 1 page of writing, but the results are virtually identical if we change this threshold.

[25] We thank Giacomo Ponzetto for raising this point.

report the results in Table A.4 of the Online Appendix.[26] We find no evidence consistent with differential changes in legal standards—the coefficients change sign across definitions of litigation, are economically small, and statistically insignificant.

VII Which Clauses Are Added When Trust Dops?

As we discussed in Section III.*B.*, higher contract complexity only implies higher completeness if the additional clauses in the contract refer to verifiable contingencies, but our text-analysis based measures cannot inform us on the verifiability of contract clauses. Our analysis so far has assumed that at least one of the clauses added to contracts refers to a verifiable contingency. To better understand whether contracts become more complete after the negative shock to trust we study 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.[27]

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 8 we report the estimates of the difference-in-differences coefficient for estimating equation (2) 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 and hence we can reject the null that the topic was not more likely to be added in contracts signed after 2002 in high-AA states.

[26] We report these results in the appendix because the level of observation of this analysis (state-year) and the underlying data differ from the rest of our empirical tests in the paper.

[27] 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.

To assess these topics qualitatively, in Figure 6 we show the cloud representation of 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 the LDA procedure does not require that the number of words that appear related enough to constitute a topic is the same across all topics and at the same time to guarantee that the graphical representation is readable, we do not report the words whose probability is quite low and hence whose font would be so small that it cannot be read. Some clouds depicted in Figure 6 appear to include less words than others for this reason. Topics related to more sparse clouds include many unreported words whose probability of being part of the topic is quite small.

A common theme across the 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. Under this common theme, 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"), to agents' obligations ("obligation," "agents," "compliance," "responsibility"), and specifically to the state of the world in which information is disclosed by agents to third parties ("disclosure," "confidentiality," "damages," "liability").

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 6. 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 whose presence we detect increasing in columns (6)-(8) of Table 8 are

non-compete agreements.[28] We estimate the specification in equation (2) 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 8 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 enforcing states is quite low—the correlation between the continuous share of AA clients 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.[29]

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

[28] 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.

[29] We thank Rocco Macchiavello for suggesting this test.

seems rather implausible.

VIII 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 trust on contract completeness do no fade throughout our sample period. Moreover, our qualitative analysis of the specific clauses that tend to be added to contracts after the negative shock to trust reveals that such additions 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 pressure on managers to complete the contracts signed with (untrustworthy) agents, by managers' intent to insure themselves against possible actions of shareholders after a damage caused by agents—actions that were foreseeable before and after the shock and that managers could have provided for in contracts irrespective of the shock—and/or by distrustful agents who want to put in writing the conditions under which their future actions might be deemed in violation of the contract are three non-mutually exclusive channels that we cannot disentangle in our analysis and 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. A contribution of our paper is to provide empirical measures of contract completeness based on textual analysis techniques that can be used broadly to bring predictions of contract theory to the data.

Our results also open paths for other research avenues. Understanding the dynamics of the effects of trust on contracting—e.g., whether trust evolves and builds up after continued interactions between parties and more information is produced—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. Whereas more complete contract presumably require spending higher costs of negotiation by both parties and restrict agents' actions, a different setting is needed to estimate comprehensibly the economic effects for both parties and competitors.

Also, how does trust relate to other contract features that are specific to certain classes of contracts and sectors? For instance, Gennaioli, La Porta, Lopez-de Silanes, and Shleifer (2020) study the effects of trust and honesty on the premia and damage payments of insurance contracts. And, could private interventions (Barone et al. (2015)) and automated interventions such as robo-advising tools (D'Acunto et al. (2019), D'Acunto and Rossi (2020), D'Acunto and Rossi (2021)) avoid the effects of a drop in trust among counterparties on contract negotiations and design?

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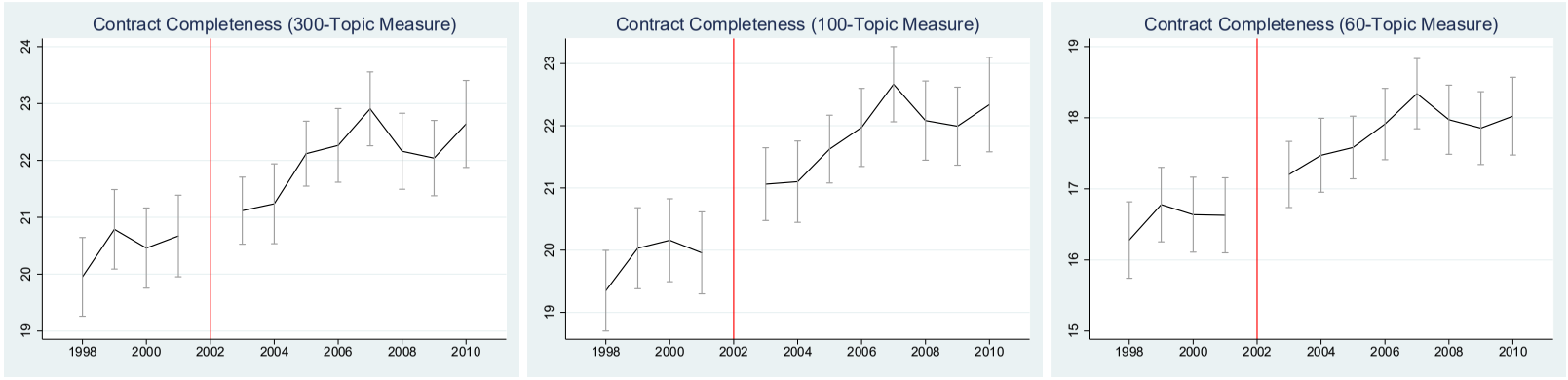
Figure 2: **Contract Completeness over Time: Other Measures**

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

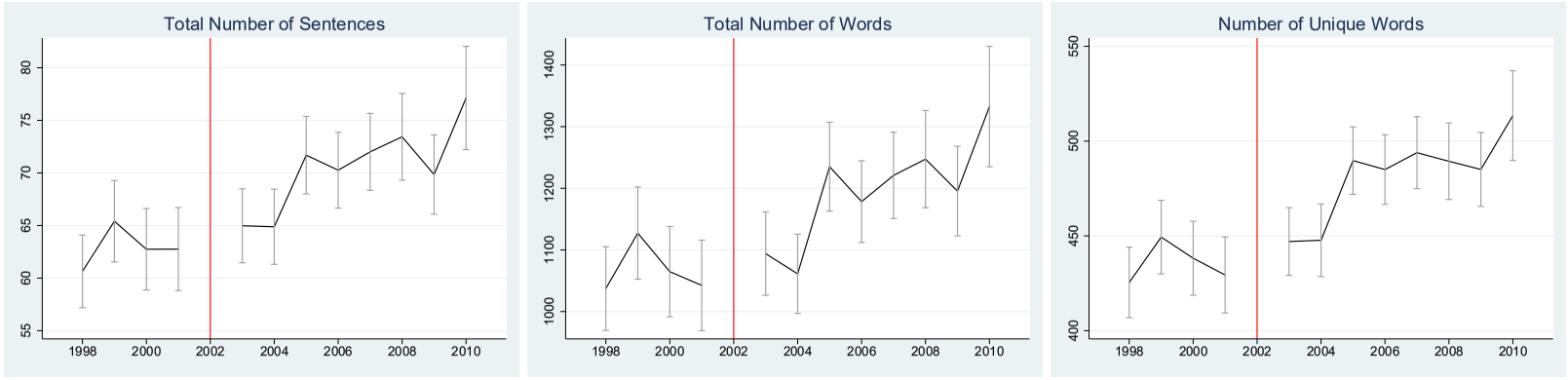
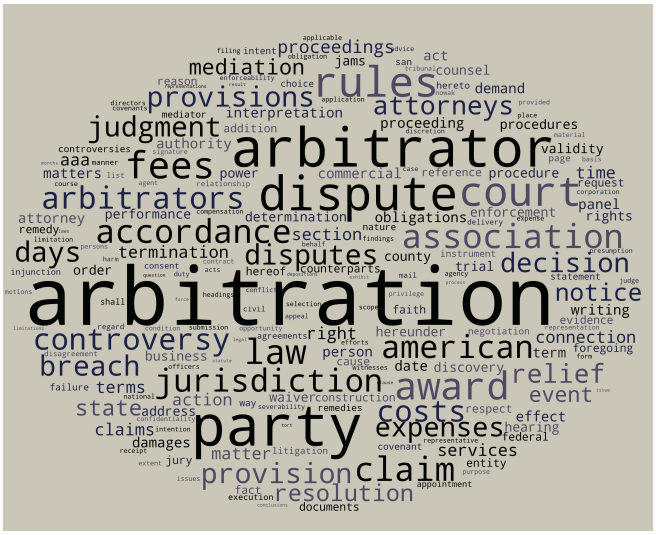


Figure 3: **Examples of LDA Topics Identified in Consulting Contracts**

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 C.1.

A. Arbitration to Solve Controversies Between Parties



B. Relationship to Company's Board Members

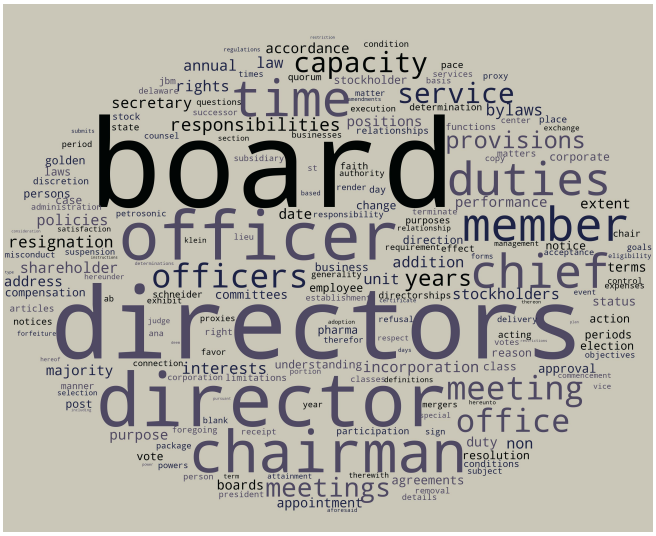


Figure 4: **Geographic Variation in the Share of AA Clients in 2000**

This figure is a state-level heatmap for the share of listed companies in each state that were AA clients 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.

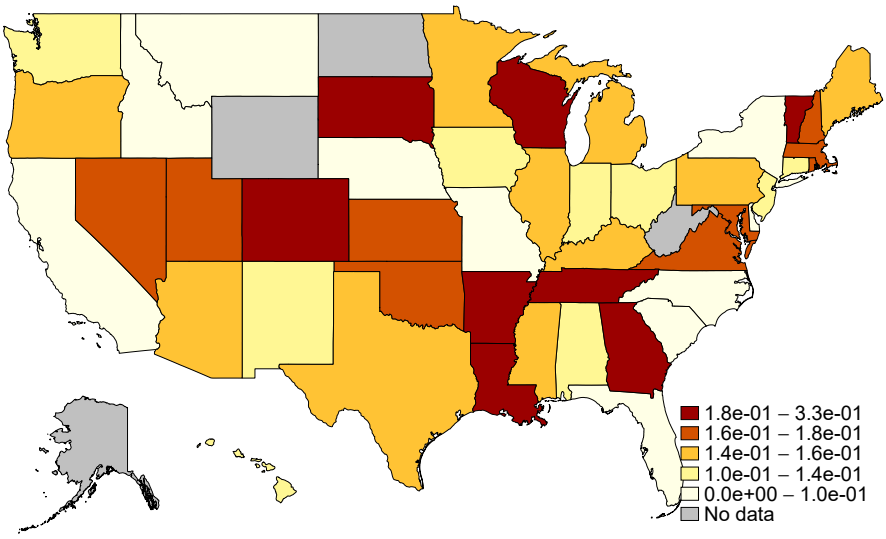


Figure 5: **Pools of Contracting Firms and Consultants Over Time**

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

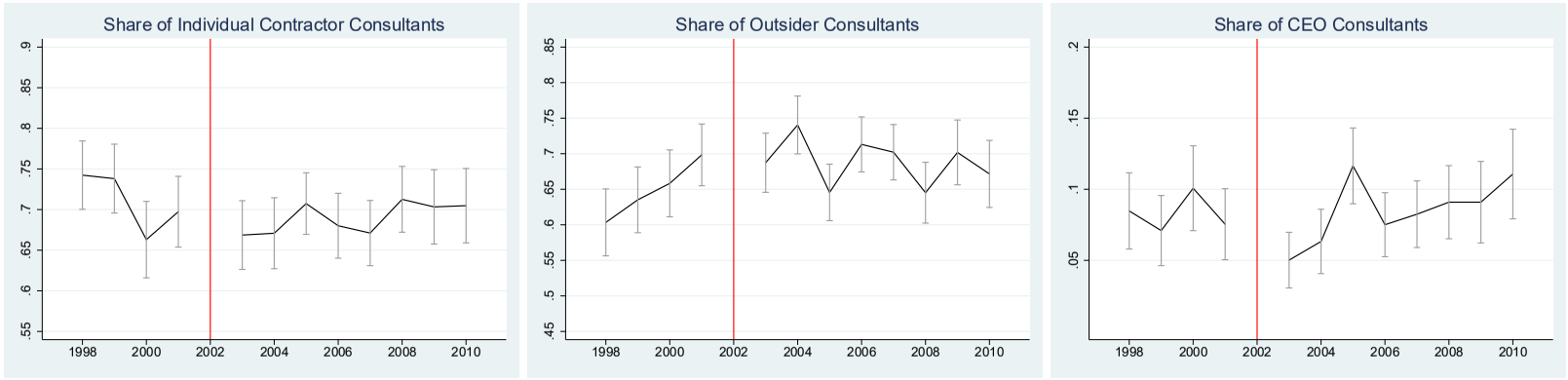


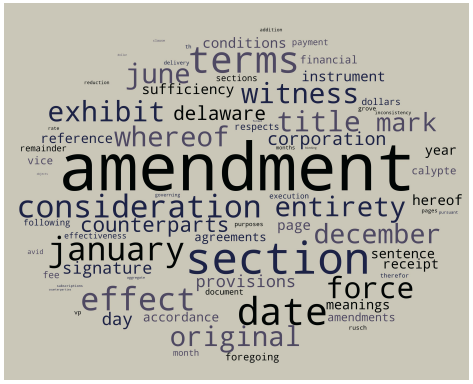
Figure 6: **Topics Appearing More Often in High-AA States After the Negative Shock to Trust**

This figure reports the cloud representation of the LDA-based matrix of terms and probabilities underlying the topics whose likelihood of appearing in our sample's contracts after 2002 in high-AA-exposure states is highest.

A. Confidentiality & Data Secrecy

B. Contract Termination

C. Contract Amendments



D. Indemnification

E. Restriction Equity to Agent

Table 1: **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 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 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. # Words refers to the total number of words of each consulting contract. # Unique Words refers to the total number of unique words of each contract. # Sentences refers to the total number of sentences of each contract. 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. Option is a dummy variable that equals 1 if a firm grants stock option to the consultant, and zero otherwise. Equity is a dummy variable that equals 1 if a company compensates a consultant with company shares. Option/Equity is a dummy variable that equals 1 if either stock option or equity compensation is included in the 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. 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 is disputes arise, and zero otherwise. Return is the state-level annualized stock returns (weighed by firm capitalization) over the past 12 months upon contracting. Volatility is the state-level standard deviation of market-adjusted returns (weighted by firm capitalization) over the past 52 weeks upon contracting, and zero otherwise. 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. Number of contracts per firm							
	Total	Mean	std	Min	p25	p50	p75	Max
# Total	6081	3.310	3.581	1	1	2	4	32
# Total, post=0	1931	2.177	1.808	1	1	2	3	13
# Total, post=1	4150	3.087	3.159	1	1	2	4	25
# Outsider	4067	3.312	3.754	1	1	2	4	32
# post=0, Outsider	1271	2.128	1.730	1	1	2	3	13
# post=1, Outsider	2796	3.139	3.280	1	1	2	4	25
# Insider	2014	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	1354	1.811	1.514	1	1	1	2	13
# Public	4671	2.776	2.813	1	1	2	3	24
# post=0, Public	2736	2.623	2.634	1	1	2	3	18
# post=1, Public	1935	2.234	1.984	1	1	2	3	13
# Private	1410	3.417	3.001	1	1	2	5	16
# post=0, Private	1071	3.088	2.321	1	1	2	5	10
# post=1, Private	339	2.510	2.437	1	1	1	3	11

	Panel B. Contract-level characteristics							
	Total	Mean	Std	Min	p25	p50	p75	Max
AA%	6081	0.133	0.043	0	0.092	0.139	0.160	0.333
EY%	6081	0.144	0.044	0	0.110	0.150	0.182	0.235
Deloitte%	6081	0.106	0.031	0	0.087	0.104	0.113	0.364
KPMG%	6081	0.103	0.030	0	0.089	0.107	0.120	0.211
PWC%	6081	0.149	0.044	0	0.120	0.141	0.189	0.385
Post	6081	0.626	0.484	0	0	1	1	1
Public	6081	0.768	0.422	0	1	1	1	1
Outsider	6081	0.669	0.471	0	0	1	1	1
Completeness 200	5909	22.960	8.050	3	18	22	28	71
Completeness 100	5909	21.115	7.097	3	16	21	25	62
Completeness 300	5909	21.501	7.472	2	17	21	26	76
# Words	5909	1167.741	970.474	100	609	972	1448	19439
# Unique words	5909	465.083	216.539	48	314	443	585	2038
# Sentences	5909	68.537	50.921	3	40	59	84	1067
Option	6081	0.316	0.465	0	0	0	1	1
Equity	6081	0.422	0.494	0	0	0	1	1
Option/Equity	6081	0.605	0.489	0	0	1	1	1
Non-compete	6081	0.299	0.458	0	0	0	1	1
Duration	4949	20.785	20.237	0.200	11.100	12.167	24.333	240
Renegotiation	6081	0.035	0.183	0	0	0	0	1
8K	6081	0.318	0.466	0	0	0	1	1
Company	6081	0.293	0.455	0	0	0	1	1
Arbitration	6081	0.292	0.455	0	0	0	1	1
Return	6081	0.132	0.237	−0.737	0.008	0.139	0.278	1.424
Volatility	6081	0.050	0.020	0	0.036	0.044	0.060	0.164
Ln(GDP)	6081	13.112	0.951	9.750	12.451	13.113	13.919	14.755

Table 2: **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:

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

where $Trust_{i,s,t}$ is a dummy variable that equals 1 if a respondent i in state s reports a great deal or quite a lot of confidence in US big businesses in year t, and zero otherwise in columns (1)-(7), and confidence in the institutions reported on top of each column in columns (8)-(10). In columns (1)-(3) and (8)-(10), $Treated_s$ is a dummy variable that equals 1 if respondent i resides in a state in which the share of local public companies clients of AA in 2000 was in the top 25% of the distribution, and zero otherwise. In columns (4)-(7), $Treated_s$ is a dummy variable that equals 1 if respondent i resides in a state in which the share of local public companies clients of each of the other Big Five in 2000 was in the top 25% of the distribution, and zero otherwise. $After$ 2002 is a dummy variable that equals 1 if the survey was conducted in 2003 or later, and zero otherwise. White is a dummy variable that equals 1 if the respondent declares himself/herself to be white, and zero otherwise. Black is a dummy variable that equals 1 if the respondent declares himself/herself to be black, and zero otherwise. Republican is a dummy variable that equals 1 if the respondent declares himself/herself to be a republican, and zero otherwise. Protestant/Jewish is a dummy variable that equals 1 if the respondent declares himself/herself to be either Protestant or Jewish, and zero otherwise. Male is a dummy variable that equals 1 if the respondent is male, and zero otherwise. Ln(age) is the logarithm of the respondent's reported age. Married is a dummy variable that equals 1 if the respondent declares himself/herself to be married. High Income is a dummy variable that equals 1 if the respondent with household income above the US median household income. We cluster standard errors at the level of the state (k). We weigh observations by the number of total observations within the same state to overweigh more representative responses. The sample period is 1994 to 2015.

	Top 25% Share AA in 2000			**Top 25% Share Big 5 in 2000**				**Trust in Other Institutions**		
				EY	Deloitte	KPGM	PWC	Small Businesses	Judicial System	Banks
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
After 2002	-0.045***									
	(0.008)									
Top 25% × After 2002	-0.041**	−0.037**	−0.036**	−0.023	0.001	0.007	0.022	−0.018	−0.002	−0.002
	(0.016)	(0.015)	(0.016)	(0.019)	(0.024)	(0.015)	(0.014)	(0.048)	(0.024)	(0.017)
Top 25%	0.022**	0.017*								
	(0.010)	(0.009)								
Constant	0.314***	0.300***	0.277***	0.274***	0.269***	0.267***	0.264***	0.641***	0.385***	0.332***
	(0.042)	(0.045)	(0.052)	(0.058)	(0.056)	(0.057)	(0.056)	(0.055)	(0.089)	(0.077)
Individual Controls	X	X	X	X	X	X	X	X	X	X
Year FE		X	X	X	X	X	X	X	X	X
State FE			X	X	X	X	X	X	X	X
N	15130	15130	15130	15130	15130	15130	15130	6179	13092	15130
adj R^2	0.059	0.062	0.062	0.057	0.057	0.057	0.057	0.039	0.024	0.068

Table 3: **Trust and Contracting**

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

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

where $Completeness_{i,j,p,k,t}$ measures completeness of contract i signed by firm j in state s 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. The construction of this measure and similar measures for robustness is described in section III. We estimate the specification using a negative binomial estimator 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). 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. Please refer to Table 1 for definitions of other variables. We cluster standard errors at the level of state (s). The sample period is from 1994 to 2015.

	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	2772	2772	2772	2772	2772	2772
Pseudo R-sq	0.007	0.017	0.031	0.007	0.017	0.031

Table 4: **Trust and Contracting: Robustness**

This table provides various robustness check of the results in Table 3. Across different panels, dependent variables take a variety of alternative measures for contract completeness. In Panel A, the dependent variable is Completeness200 but we perform weighted negative binominal regressions. We assign the number of public firms in each state k as of contracting year t as the weight. In Panel B, we perform OLS regressions and completeness is measured as the number of topics of Completeness200 divided by 200. In Panel C, 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 D and E, we perform negative binomial regressions and dependent variables are Completeness100 and Completeness300, respectively. In Panel F, G, and H, we perform negative binomial regressions and completeness is measured as the total number of words, sentences, and unique words of a contract, respectively. Please refer to Table 1 and Table 3 for definitions of other variables. We cluster standard errors at the level of state (k). The sample period is from 1994 to 2015.

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. 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^2	0.008	0.016	0.031	0.008	0.016	0.031
	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^2	0.036	0.067	0.155	0.037	0.067	0.155
	Panel C. OLS, Dummy Completeness 200 Above Meidian					
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^2	0.025	0.049	0.105	0.025	0.048	0.104
	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^2	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^2	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^2	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^2	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^2	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	2772	2772	2772	2772	2772	2772

Table 5: **Trust and Contracting: Falsification Tests**

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

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

where $Completeness_{i,j,p,k,t}$ measures completeness of contract i signed by firm j in state s 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. The construction of this measure and similar measures for robustness is described in section III. We estimate the specification using a negative binomial estimator 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 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 measures computed for the Big 5 consulting company listed 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 capture the length of the contractual relationship. Please refer to Table 1 for definitions of other variables. We cluster standard errors at the level of state (s). The sample period is from 1994 to 2015.

	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	1766	2772	2772	2772	2772	1766	2772	2772	2772	2772
Pseudo R-sq	0.037	0.031	0.031	0.031	0.031	0.037	0.031	0.031	0.031	0.031

Table 6: **Trust and Contracting: IV Specifications**

This table reports estimates of two-stage least-square IV specifications with the following first and second stage:

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

$$Completeness_{i,j,s,p,t} = \alpha + \beta\ \widehat{Trust}_{i,j,s,p,t} + X'_{j,s,p,t}\delta + \eta_t + \eta_p + \epsilon_{i,j,s,p,t},$$

where $Trust_{i,j,s,p,t}$ is the share of Gallup Survey respondents who report to trust business practices in each state s and year t in which contract i signed by firm j in industry p is signed. In columns (1)-(3), this measure is based on the answers of college-educated survey respondents (who are more likely to be shareholders through holding stocks of listed firms), whereas in columns (4)-(6) it is based on the whole set of survey respondents. $Completeness_{i,j,p,k,t}$ measures completeness of contract i signed by firm j in state s 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. The construction of this measure and similar measures for robustness is described in section III. $Treated$ 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. $After\ 2002_t$ is a dummy variable that equals 1 if the contract was signed in 2003 or later, and zero otherwise. In columns (2) and (5), the excluded instrument for $Trust$ is only the interaction between $Treated$ and $After\ 2002$, whereas in columns (3) and (6) both the interaction and the level of $Treated$ are used as instruments for $Trust$. Please refer to Table 1 for definitions of other variables. We cluster standard errors at the level of state (s). The sample period is from 1994 to 2015.

	State-year Trust (College Educated)			**State-year Trust (All Respondents)**		
	First Stage	Second Stage One instr.	Second Stage Two instr.	First Stage	Second Stage One instr.	Second Stage Two instr.
	(1)	(2)	(3)	(4)	(5)	(6)
Treated ×	-0.590**			-0.492**		
After 2002	(0.251)			(0.230)		
Treated	-0.192	−0.003		-0.168	−0.003	
	(0.235)	(0.008)		(0.151)	(0.007)	
Trust		−0.025**	-0.020**		−0.028*	−0.022*
		(0.011)	(0.010)		(0.015)	(0.013)
8K Reporting	0.041	0.007***	0.007***	0.027	0.007***	0.007***
	(0.066)	(0.002)	(0.002)	(0.040)	(0.004)	(0.002)
Company	0.209**	0.018***	0.017***	0.147**	0.017***	0.016***
	(0.087)	(0.004)	(0.004)	(0.063)	(0.004)	(0.003)
Arbitration	-0.010	0.027***	0.027***	-0.023	0.027***	0.027***
	(0.099)	(0.003)	(0.003)	(0.074)	(0.003)	(0.0003)
Return	-0.029	−0.007	-0.006	0.048	−0.009	−0.008
	(0.649)	(0.020)	(0.018)	(0.596)	(0.021)	(0.018)
Volatility	-10.71	0.028	0.077	-7.654	0.036	0.080
	(10.04)	(0.344)	(0.255)	(7.921)	(0.307)	(0.236)
Ln(GDP)	-0.298	−0.005	-0.004	-0.252	−0.005	−0.004
	(0.229)	(0.008)	(0.005)	(0.186)	(0.008)	(0.005)
Constant	2.815	0.133	0.118	1.907	0.121	0.106
	(3.399)	(0.104)	(0.073)	(2.570)	(0.104)	(0.071)
Year FE	X	X	X	X	X	X
Industry FE	X	X	X	X	X	X
N	2667	2667	2667	2706	2706	2706
First-stage F-stat		5.53	6.58		8.27	5.10

Table 7: **Trust and Contracting: Alternative Explanations**

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

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

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 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 3. We cluster standard errors at the level of state (s). The sample period is from 1994 to 2015.

	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	2772	2772	2772	2772	2772	2772
Adjusted R-sq	0.011	0.034	0.042	0.013	0.034	0.041
	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	2304	2304	2304	2304	2304	2304
Adjusted R-sq	0.026	0.047	0.057	0.026	0.047	0.057
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 8: **Which Clauses Appeared More Frequently After the Shock to Trust?**

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

$$Y_{i,j,p,k,t} = \alpha + \beta\ Treated_s \times After\ 2002_t + X' \times \gamma + \phi_k + \phi_p + \phi_t + \epsilon_{i,j,p,k,t}$$

where $Y_{i,j,p,k,t}$ 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 industry p and state k as of year t. The words that compose each topic are depicted in Figure 6. $Treated$ 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 1 for definitions of other variables. We cluster standard errors at the level of state (k). The sample period is from 1994 to 2015.

	Confidentiality & Data Secrecy	Termination	Contract Amendments	Indemni-fication	Restrictions to Equity	Non-compete Clauses All	Non-compete Clauses Enforcement	
							High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated × After 2002	0.077**	0.967**	0.570*	0.456*	1.005**	0.503	1.454***	−0.295
	(0.035)	(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	2772	2772	2772	2772	2772	2772	1341	1329

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**

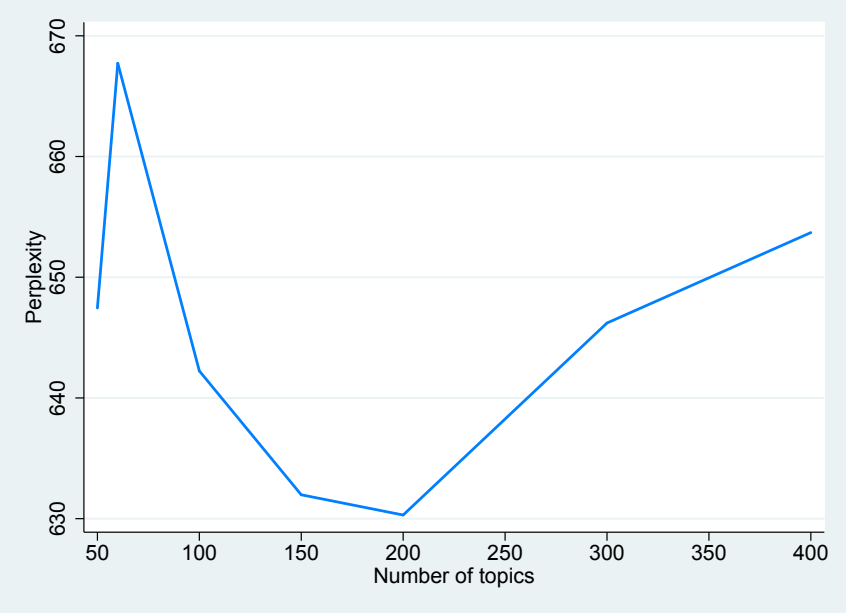


Figure A.3: **Trust or Changing Disclosure Requirements after SOX? Number of Contracts Disclosed Per Firm**

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

Table A.4: **AA Scandal and Civil Lawsuits Involving Contract Violations**

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 1 averaged at the state-year level. We cluster standard errors at the level of state (s). 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	1271	1271	1271	1271	1271	1271
Pseudo/Adj. R-sq	0.269	0.232	0.654	0.267	0.229	0.660